SCIENTIFIC AMERICAN SUPPLEMENT. No. 1891

Entered at the Post Office of New York, N. Y., as Second Class Matter
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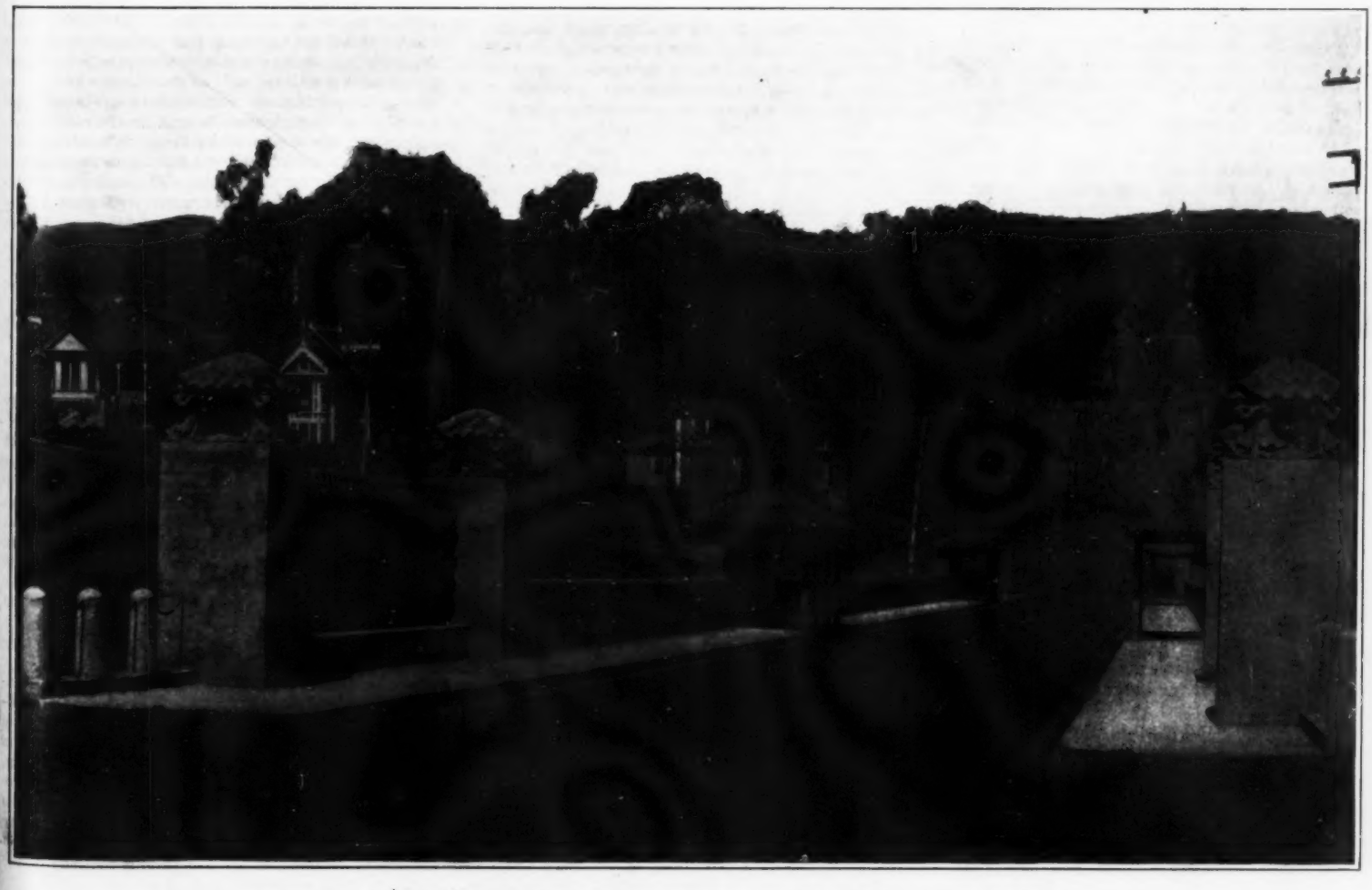
Published weekly by Munn & Co., Inc., at 361 Broadway, New York
Charles Allen Munn, President, 361 Broadway, New York.
Frederick Converse Beach, Sec'y and Treas., 361 Broadway, New York.

Scientific American, established 1845.
Scientific American Supplement, Vol. LXXIII., No. 1891.

NEW YORK, MARCH 30, 1912.

Scientific American Supplement $5 a year.
Scientific American and Supplement, $7 a year.

This structure preserves the simplicity and quaintness of the old mission type of architecture.



At the portals of the bridge are four pylons capped with concrete decorations enclosing electric lamps.

A CONCRETE BRIDGE OF STRIKING DESIGN.—[See page 200.]

Time and Space in Modern Physics*

In the Light of the Principle of Relativity

By Prof. L. Mosbacher

It has been shown as the result of recent investigations, especially in the field of electricity and heat radiation, that a number of our fundamental premises are not by any means exact. Thus, for instance, it is impossible, as has hitherto been tacitly assumed, to clearly distinguish the internal energy of a body depending on its density, temperature and chemical constitution, from its kinetic energy, as soon as the body is in motion, since such motion affects the internal energy, in particular increasing the temperature. Furthermore, the inert mass of a body is defined in terms of the kinetic energy, hence, the mass of a body also, in so far as it determines the resistance to motion, is incapable of absolute determination; in fact, it increases with increasing velocity.

Thus the concept of mass which hitherto was regarded as fundamental loses something of its original significance.

Furthermore, the identity hitherto assumed to exist as regards ponderable and inert mass can no longer be admitted; for it has been found that the inertia of the body depends on a number of circumstances, in particular, on its temperature, while gravitation is independent of these factors. In the same way there are a number of other fundamental elements of the old system that are tottering, as will be seen further on.

It is true that the departures from the hitherto assumed exact relations are in many instances very small, or else occur only in conjunction with such high velocities that for ordinary purposes of everyday *practical* life they may *for the present* be left out of the question and the old system of mechanics will still hold its sway.

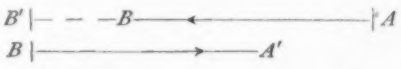
Pure science, however, cannot ignore these facts, and it appears not beyond the range of possibilities, judging from our present experience, that the exact development of pure physics will bring in its train practical consequences.

We must, therefore, look to a new foundation for physics: Prof. Max Planck[1] has shown that such foundation can be obtained by taking for our basis the so-called "principle of least action," whose application extends through the whole realm of physics, and the "principle of relativity," whose formulation in its most general setting we owe to Einstein.[2]

It goes without saying that the second principle also must have a very extended scope, and we shall see that it gives us our physical conception of space and time, or, to be more exact, the measurement of space and time.

The principle itself has long been known. We note, for example, that upon a ship proceeding over the ocean with uniform speed (and without rotating), the motion is imperceptible to an observer from the ship in mid-ocean, and every process on board proceeds exactly in the same manner as if the ship were at rest; in particular, the motion itself can be detected only by reference to the change in appearance of any coast line which may be in sight. In fact, the statement may be made *quite* generally: every process upon a body moving with uniform motion appears to an observer sharing this motion exactly the same as if the body were at rest, or, in other words, "it is impossible by any means whatever to demonstrate the uniform motion of a body relatively to empty space." In this form the principle has long been known.

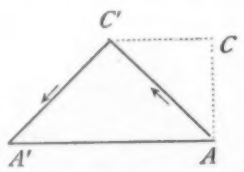
Einstein extended the principle by the further assumption that we must admit the existence of an absolute velocity—the velocity of light. How do we, for example, regulate the time between two points A and B? Let us make an imaginary experiment. We send a light signal from A to B; let this light impinge at B upon a mirror and be reflected back to A. If we halve the time $2t$ which elapses between the instance of sending off the signal and its return to A, this gives us the interval t required by the light to traverse the path AB. The light therefore arrives at B by this interval later than it started from A. If, therefore, we add this interval t to the time T observed at A at the moment the signal is sent out, and set the clock at B to indicate the time $T+t$ so obtained at the instant B receives its signal, a clock thus regulated at B indicates the same times as the clock at A. So long as the earth is considered at rest this point of view is free from objections. As a matter of fact, however, the earth performs a number of different motions; it rotates about its axis, revolves round the sun and with the latter traverses a certain orbit in space. Now let us consider the imaginary experiment discussed above, but taking into account the earth's motion, though considering, for the sake of simplicity, that the sun is at rest. Let A and B be two points on the earth's orbit. While the light travels from A to B the earth has advanced from B to B'. Hence, the path traveled by the light is longer than AB. On the return of the signal A has arrived at A', so that the return path $A'B'$ is shorter than AB. The changes in this pa h depend on the velocity of the earth and on the time taken by the light to travel. A simple calculation shows that in these circumstances the time observed as being required for the light to travel to and fro is longer than if the earth were at rest, and is the longer the greater the earth's velocity.



Now since the true velocity of the earth is absolutely unknown to us, this experiment shows that the origin of time in B cannot be regulated from A, since it is impossible to determine the time required by the light to travel from A to B and back, so that the clock at B cannot be regulated by that at A.

Thus, every point in space has its own origin of time, independently of every other point in space, and in particular, simultaneity for two points in space has no definite signification, but can be given a meaning only by a special combination.

Let us now return to the experiment in which the earth simply rotates about the sun, and make the experiment between the point A and the point C, located at the same distance from A as B was, but in a different direction with respect to the earth's orbit. For instance, AC might be taken at right angles to the earth's orbit. Then, if, during the outward journey of the light, C passes to C', and during the return journey of the light, A passes to A', the total travel of the light is now $AC'+C'A'$, and is, therefore, different from that previously supposed, and the same kind of conclusion is reached for any other direction. Calculation now shows another, a shorter time than before.



If we were able to demonstrate these differences in time in different directions, we would have proved the earth's absolute motion experimentally, and the principle would be wrong. Michelson's classical experiment corresponds essentially to the imaginary experiments discussed above, and places beyond question that the differences which should occur here if the principle of relativity did not hold would be within the limits of measurement.

Now, experiment shows that no matter in what direction the measurements are taken, in every instance the same result is obtained, and that the different directions do not show any difference. Thus it is proved experimentally that an observer moving with a system is incapable of detecting the changes produced by the motion, and thus the principle of relativity, as enunciated by Einstein is experimentally confirmed.

How then, must we frame our ideas of time and space, in order that the same result should always be attained, no matter in what direction the observation is made? The remarkable thing is that in the different directions the distances traveled and the times are different, and yet in combination they always lead to the same result. Now mathematics shows that under certain assumptions this is possible, and a simple calculation of the nature of a transformation of co-ordinates suffices to show that there exists for every direction and every point of a moving body a corresponding change of the space and time scales of the observer moving with the object, such that the results described above exactly follow, so that the observer remains entirely ignorant of the change. Space and time in this matter play exactly similar rôles and mathematically are indistinguishable from one another, especially in the presentation given by Minkowski,[3] and since the new system lays a severe tax on the powers of imagination, it is probably best to follow a purely mathematical presentation in order to protect ourselves from logical errors.[4]

The following reflections may perhaps serve to give some insight into the state of affairs. Time and space are measured by an observer moving with the object, to whom they appear different than they do to a stationary observer. Mathematical formulæ show that distances and times both appear longer. This is somewhat at variance with our accepted notions, but there is nothing illogical about it, and we have to accustom ourselves to the idea. That the different directions of observation have no effect upon the result is explained by the fact that the time and space magnitudes so compensate for one another that the result remains the same for us, as is shown by mathematics.

A crude example may illustrate this: Let us suppose that a certain distance appears to us three times as long in a certain direction as in another, and also that the time observed for travel in the first direction appears three times as long as in the other. Then the velocity of motion (distance divided by time) will be the same in both cases and would therefore have suffered no change through the change in direction of observation. Prof. Cohn of Strasburg has devised a model which demonstrates the conception of time and space of the observer moving with the object, and of a stationary observer.

The chief difficulty is to realize the relativity of time and the fact that it depends just as much as space on the observer, and that time and space must be considered of one category. It is worth while briefly to consider some of the most important consequences of the principle of relativity. In the first place, the ether, that stumbling-block of physics, becomes superfluous. The impossibility of the ether moving along with matter has been previously demonstrated, while, on the other hand, no motion of the earth relative to a stationary ether can be demonstrated. Hence, the ether is superfluous. With it the mechanistic conception of nature falls to the ground.

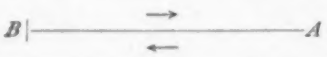
To return to the considerations of the opening paragraph: Which are the elementary constants of physics? Evidently only those magnitudes which are independent of the earth's motion, such as the velocity of light in vacuo, the elementary electric charges, entropy and several other magnitudes. Energy, on the other hand, and velocity are dependent on the earth's velocity and are, therefore, not exactly proportional to the observed velocities of the moving bodies. The inert mass determined by them is, therefore, also not a constant. Similarly, the time of swing of the pendulum, and with it the rate of all clocks, depends on the earth's motion. It is therefore impossible by mere clocks to regulate to unison time at different points in space, since the clocks have a different rate at different places.

The theorem of the parallelogram of forces is only approximately true.

Certain phenomena, such as the velocity of light in a moving liquid, which hitherto could not be explained at all, or only with far fetched artifices, are very simply accounted for by the principle of relativity.

It was shown by Fizeau in an ingenious experiment that this velocity does not depend in any simple manner on the velocity of the moving liquid. This is immediately obvious from the point of view of the principle of relativity. Let c be the velocity of light in the liquid at rest. Then, according to the principle of relativity an observer moving with the liquid must find the same velocity, and hence, the observer situated outside the liquid will find another smaller velocity differing from c, which can be determined mathematically, and is found to be in accord with the one experimentally observed.

The principle of relativity discloses to us the conception of time and space independent of the standpoint of the observer, and thus one step forward is made in freeing physics from anthropomorphism, a result which, according to Planck, is the highest aim the physicist can set before himself. On the other hand, from the point of view of biology, it is also of the highest significance that, as the principle of relativity shows, our mental interpretation of the processes which are highly complicated by the various motions of the earth, is itself so simple.



* Translated for the Scientific American Supplement from *Die Umschau*.

[1] Sitzungsbericht der preussischen Akademie der Wissenchaften, June 13th, 1907.

[2] Jahrbuch der Radioaktivitat, vol. 4.

[3] Zeit und Raum. Physikalische Zeitschrift, 1911.

[4] Planck, Columbia-Lectures. Leipzig, 1910.

Aluminum in the Brewing Industry*

Its Use for Fermenting and Storage Vessels

By C. Bleisch

ALUMINUM as a material for equipping the fermenting and storage cellars has for a long time excited great interest in technical circles. And this interest is justified, since, if this metal, distinguished by its great indifference to most chemical attacks, really fulfills the requirements of sufficient resistance to fermenting beer, it would be an ideal material, because, in contrast to the other substitutes for wood, it requires no isolating layer between the beer and metal. The practical world has become friendly to the new material within a relatively short time. A factory which is engaged in the manufacture of aluminum vessels for the fermenting and storage cellars has to date delivered 880,000 gallons of capacity.

The aluminum metal now on the market is a satisfactory article in its purely mechanical aspects, and in the course of years the quality has been considerably improved. Within recent times the commercial metal shows 99.5 per cent purity. The previous treatment of the metal is of great importance, and much has been learned in this regard within the last few years; for example, it is known that cold-rolled or cold-drawn aluminum does not satisfy the requirements of the brewing industry. It is known to-day that a subsequent warming of the aluminum up to 450 deg. Cent. alters the metal in a very favorable sense.

Although these facts have not remained unknown, the very thorough researches of the Royal Institute for Testing Materials, in Berlin, published a short time ago, have pointed out a possibility of a corrosion of aluminum and has caused much unrest in manufacturing circles which are interested in the production of aluminum vessels for breweries, and also in brewing circles.

We believe that this unrest is without foundation.

The experiments were carried out on cooking vessels and other utensils, which, as should be distinctly borne in mind, were not made of the same material, either chemically or mechanically, as the material used for fermenting vessels. Although such material when immersed in distilled water for from 48 to 210 days showed corrosion and loss of weight, this is no indication that carefully manufactured, high-grade aluminum will not answer for the purposes of our industry. Iron, which is quite subject to attack, finds the widest use in practical brewing operations, and it may be said that if small traces of aluminum pass into the fermented product these traces are much less to be feared in beer than traces of iron.

The question as to whether aluminum is suitable for brewery use can only be answered after a consideration of practical conditions; and since in the investigations of the Royal Institute for Testing Materials these conditions were not considered, it appears that an investigation as to how aluminum would behave under the conditions of practice is in order.

The material experimented upon, for which we have to thank a certain factory, was used in pieces having a total surface of 60 square centimeters, being about 2 millimeters and weighing about 24 grammes. The analysis of this material gave the following results:

Aluminum	99.52 per cent
Iron	0.261
Silicon	0.21
Zinc	Trace
Copper	Small trace
Carbon	Trace

The following experiments deal with the behavior of aluminum toward water, and above all, toward fermenting worts and cleaning and disinfecting agents, which in practice are the most important substances which come in contact with the interior surface of aluminum vessels:

I. BEHAVIOR OF ALUMINUM TOWARD WATER.

Aluminum plates of the above-mentioned dimensions were kept for five weeks in contact with distilled water and ordinary water contained in breakers. The plates of aluminum were allowed to project above the surface of the water in order to ascertain whether the double action of water and air upon the metal was in any way pronounced. The well-dried pieces of metal were weighed before and after the experiment. The greatest decrease in weight was observed in the case of distilled water, but this was only 0 0005 gramme. This, however, practically lies within the limits of error of weighing. If, however, we assume that this decrease represents a natural loss we may make the following calculations:

The total surface of aluminum amounted to 60 square centimeters. This would mean a loss of 0.0835 gramme, or $\frac{0.0835}{35} = 0.00239$ gramme decrease per day per square meter of surface. No corrosion could be observed. Good Thüringer glass yields to water 0.1 gramme per day per one square meter surface This is 40 times as much as was lost by the aluminum used in these experiments. The ordinary water caused a slight increase of weight after 3½ weeks. This was found to be due to the fact that some carbonate of lime had been deposited from the hard water, and this was the cause of the relatively small increase of weight. It might be objected that the experiments should have been carried out for a longer time, in which the decrease of weight would probably have been somewhat greater. However, we do not concur in this view, since in the brewing industry pure water remains in contact with the aluminum only during the process of washing, and it soon gives place to the fermenting beer.

Pieces of aluminum were also moistened with water and kept in the fermenting cellar for several weeks, after which they were carefully dried and weighed. No corrosion such as might be detected by loss of weight was observed. No spots or flakiness on the surface of the metal appeared. From the results of this investigation we may conclude that aluminum of the quality used by us in our tests will not be exposed to any danger when washed for years with hard or soft water under the conditions of practice.

II. BEHAVIOR TOWARD FERMENTING WORT.

(a) *In the Fermenting Cellar.*—For this purpose pieces of aluminum of the same size as before were left for 45 days in the fermenting beer in the fermenting vat. On the average, the following changes were detected by weighing:

After 16 days, weight increased 31.8 milligrammes from beerstone.

After 22 days, weight increased 55.6 milligrammes from beerstone.

After 30 days, weight increased 85.2 milligrammes from beerstone.

After 45 days, weight increased 129.0 milligrammes from beerstone.

Recalculating these figures to one square meter of surface, we find the following amounts of beerstone deposited upon one square meter of aluminum:

After 16 days..... 5.31 grammes.
After 45 days.....21.54 grammes.

The deposit of beerstone is, therefore, extraordinarily great. Although this result would probably not be the same in all breweries, we must take account of the possibility of a stronger deposit of beerstone, which will be much greater than in the case of wooden vats.

In order to ascertain the time at which the deposits of beerstone began during the first fermentation in the aluminum vats different pieces of aluminum were brought into a fermenter (eight-day fermentation) and observed daily. The following gives the average of several experiments:

After 1 day, decrease 1 milligramme.
After 3 days, decrease 1.4 milligrammes.
After 4 days, decrease 1.4 milligrammes.
After 4 days, decrease 0.8 milligramme.
After 5 days, increase 1 milligramme.
After 6 days, increase 4 milligrammes.
After 8 days, increase 8.5 milligrammes.

From this we see that the greatest decrease in the first four days was 0.234 gramme per one square meter of aluminum surface as compared with the daily decrease of 0.1 gramme per one square meter on good Thüringer glass exposed to the action of water. It is therefore clear that only in the first days of the main fermentation occurs a loss of aluminum, which, however, is practically without significance. This is less than the loss caused by the influence of water on good glass and is followed by a rapid and extensive deposit of beerstone. This beerstone on examination was found to be completely aluminum-free, and to consist of its ordinary constituents, namely, oxalate and phosphate of lime, albumen, etc. The beerstone which adhered to the aluminum has a peculiar property, which is essentially different from the ordinary beerstone found in wooden vats. It deposits in a short time and in relatively large amount as a glassy coating on the aluminum and forms a natural and excellent insulation for the metal against the fermenting beer. It is resistant to not too strong mechanical influences, and in consequence of its very smooth surface and other peculiar properties it is surely not so dangerous as a source of infection as the beerstone of wooden vats. For this reason it is not necessary to remove it as often as usual. When removal becomes necessary this is not so easily done with mechanical means. Possibly dilute nitric acid is of value in this connection, since according to our observations a short action of this substance loosens the beerstone so that it can be easily removed without attacking the aluminum. This is naturally merely a suggestion, and only to be considered on the rare occasions when the removal of the beerstone from aluminum vats offers difficulties. Hot soda solutions or caustic alkalies, as is pointed out below, cannot be used for this purpose without careful consideration.

It is said that aluminum is attacked at the points where the kräusen comes in contact with it, namely, at the point where moisture, carbonic acid and air, and possibly also hop resins, are to be found. Such corrosion cannot be easily explained on a chemical basis, especially in the case of high-grade aluminum. Long experimenting with pieces of aluminum in this direction showed neither change in the weight of the aluminum pieces nor a corrosion These objections, therefore, we regard as without foundation when the material is really of high grade.

For the rest we know of vats which have been in use for nearly four years, and which in all respects corroborated our conclusions.

(b) *In the Storage Cellar.*—Aluminum pieces of the same dimensions as above were placed in beer four to six weeks old and kept therein for five weeks. The following changes were noted:

After 16 days, decrease of 2.6 milligrammes.
After 24 days, decrease of 1.2 milligrammes.
After 32 days, increase of 5.0 milligrammes.

The corresponding decrease therefore occurs within 16 days and amounted to 0.434 gramme per one square meter of aluminum surface, or a daily loss of 0.027 gramme, as against a daily loss of 0.01 gramme from one square meter of good Thüringia glass kept in water. Between the sixteenth and twenty-fourth day an increase in weight due to the deposition of beerstone was observed, which gradually became greater.

Next the aluminum pieces were placed in beer which had been drawn into barrels and observed for a period of 21 days. The increase—average of several experiments—was as follows:

After 15 days..... 29 milligrammes of beerstone
After 20 days..... 37 milligrammes of beerstone

This indicates 6.18 grammes of beerstone for one square meter of aluminum surface.

In the case of old beer in the storage cellar the beerstone deposits slowly. Young barrel beer, however, deposits beerstone rapidly, but even in the storage cellar the young beer soon deposits a layer of beerstone with all the desirable properties of that deposited in the vats in the fermenting cellars. However, the layer of beerstone in the storage cellar will not reach the thickness obtained in the fermenting cellar, because old beer deposits less beerstone than young. This, however, is not an important matter.

III. BEHAVIOR TOWARD DISINFECTANTS.

At the present time we use in our cellars a number of disinfectants for disinfecting the wooden vats, and of these we are able to say that considering the very smooth surface of the aluminum beerstone, these disinfectants will not be so necessary. Those, however, who intend to use them must clearly understand that many substances permissible for use in wooden vessels are not permissible in aluminum vessels. Before we begin a discussion of disinfectants in relation to aluminum we may consider caustic lime, caustic soda and ordinary soda, which are used for disinfecting purposes in wooden vats, the last chiefly for removing beerstone. These substances may be at once excluded, especially the caustic alkalies, because aluminum is very sensitive to the action of these substances. Even ordinary soda in lukewarm solutions of 8 to 10 per cent is unpermissible, because even within 15 minutes, at a temperature of 35 to 40 deg. R., this solution, when in contact with aluminum, will cause a considerable loss of weight and other indications of corrosion. Whenever it is desired to remove beerstone it will probably be best to use pure, dilute nitric acid of about 10 per cent to 15 per cent. It was shown by experiment that after treating with dilute nitric acid the metal was not more subject to attack by beer. A practical test would be desirable to test the practicability of this suggestion.

Among the direct disinfectants the following were investigated, both in the solid form and in 5 per cent solutions:

1. Antiformin (solution of hypochlorite of soda, reacting strongly alkaline). Strong action on the aluminum within 15 minutes; therefore not suitable.

2. Ammonium florid (acid ammonium salt of hydrofluoric acid). Corrosion within 15 minutes; not suitable.

3. A disinfectant "M" (very similar to Montanin; active principle hydrofluorsilicic acid). After 15 minutes strong action; absolutely unsuitable for disinfecting aluminum vessels.

4. Formol (formaldehyde). No corrosion and no loss of weight could be observed after 21 days.

From these experiments it is clear that formaldehyde

* From the *Zeitschrift fur das Gesamte Brauwesen* through *Pure Products.*

is a suitable material for disinfecting aluminum. For dissolving beerstone, alkali is out of the question unless used with great care. Doubtless these cleansing agents can be replaced by pure dilute nitric acid.

If now we draw conclusions from all the above observations, which are based upon tests under practical conditions, it may be said that the uneasiness caused by the report of the Royal Institution for Testing Materials, in so far as it relates to the use of aluminum for fermenting vessels in the brewing industry, is not justified. The material demanded for the purposes of our industry must be high-grade aluminum, which requires great care in its preliminary treatment, and on which higher requirements must be placed than aluminum used in ordinary cheap utensils. If the manufacturers deliver such a material for fermenting vessels in the brewing industry, and in consequence of its other properties it deserves our best attention.

It requires no insulation against the fermenting liquid, because in the main fermentation, and also in the early after-fermentation, the thick layer of beerstone provides an excellent protection. This beerstone has peculiarly advantageous properties, for example, its smoothness and peculiar hardness, which sharply distinguishes it from ordinary beerstone. Consequently its periodical removal is not so necessary as in the case of wooden vessels. Since both theory and practice have come to the conclusion that aluminum can be brought into contact with fermenting beer without disadvantage to the product or detriment to the metal, as appears from our experiments, the problem of installing such vessels, so as to protect the outer walls and outer influences of temperature, is not so difficult a matter and can be regarded as already solved. In our opinion aluminum is a first-class competitor with other substitutes for wood, such as iron and cement, and we should consider that it has a certain value which attaches only in part to the other materials. The chief point to consider is the price and coast of installation in comparison with these other materials, and this will decide the future of aluminum in the brewing industry. But the outlook for aluminum as a material for use in the brewing industry seems most favorable.

Vegetable Tissue Viewed by Transmitted Light.

Microphotography in the Service of Applied Science

By Our Paris Correspondent

The Wear of Steel Rails Submitted to Investigation Under the Microscope

The Same Viewed by Reflected Light.

ONE of the leading French authorities upon the microscopic structure of metals is F. Robin, of Paris. It is well worth while to take note of the special microphotographic apparatus designed by him, and his investigations by microscopic methods into the wearing qualities of steel rails are of special interest. It is necessary to examine the polished surface of the steel by reflected light, a procedure which presents some difficulty in its execution. Mr. Robin arranges the sample for examination underneath the microscope objective and illuminates it by means of an arc lamp placed on the right-hand side. The light passes through the box seen at the top of the apparatus (see figure) and thence down through the objective onto the surface of the metal viewed. The light is then reflected back through the objective, but is deflected to the left, so as to throw an enlarged image of the object on the ground glass screen of the camera. The operator thus sees a very brilliant image of the metal surface on the ground glass, and this is found much less fatiguing to the eye than viewing the object directly through the microscope. The apparatus is also thoroughly well adapted for the preparation of photographs.

For oblique illumination of metal surfaces the arc is lowered, so that the beam of light falls on a small circular mirror shown to the left of the metal block in our illustration, and is then reflected obliquely onto the specimen. Such oblique lighting is found to be very helpful in disclosing the structure of the metal, as it clearly brings out little pits and elevations on the surface of the metal. Oblique illumination is also found very useful in examining other objects, such as animal tissues, the appearance presented being very different from that seen by transmitted light.

In Mr. Robin's apparatus the piece of metal under examination is attached with wax to a table which is free to move with universal motion, being joined to the base by a ball and socket joint. A system of gears and levers is carried from this table to the observer's station, so that he can adjust the specimen while viewing it in the ground glass, and without leaving his post. The microscope is arranged to give also illumination by transmitted light when desired.

Some of the results which Mr. Robin obtained by means of this apparatus in his investigation of the wear of steel rails are well worth quoting. According to a statement made by Mr. Dufaux, with ordinary French steel the wear averages 1 millimeter (0.04 of an inch) for the passage of 100,000 trains over the rail. The surface of a rail in service becomes gradually hardened as the rail is flattened. The slowing down and stopping of trains have a special effect, apart from the flattening of the rail, causing a certain alteration of the surface of the metal. Parts which have been thus acted upon are very hard and show great resistance to wear, though they also become rather brittle. As the result of this influence the rail tends to crack in a direction perpendicular to the axis of the rail, and the latter is apt to become ruptured.

From the tests performed it appears that silicon and manganese exerts very different influences upon the properties of steel rails. According to some authorities, manganese has very little influence, while, on the contrary, the French metallurgist, Derihon, as the result of some experiments on the friction of steel against hardened steel with oil lubrication, concluded that the presence of silicon and manganese improved the wearing qualities by 100 per cent. Other tests bear out this conclusion. The matter, however, is as yet undecided.

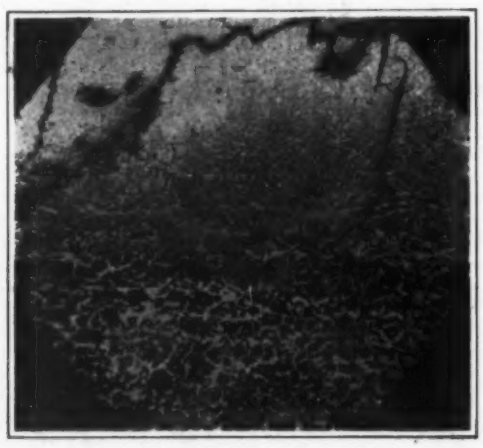
One of our illustrations is a reproduction of a microphotograph showing the alteration in the structure of the rail near the surface. Note the approximately transverse cracks. The direction of the trains is from right to left. At the top is the altered zone, which has a structure different from that of the natural metal lying underneath, and the cracks in this part are plainly visible.

The mode of illumination here discussed will find useful application not only in metallurgy but also in other branches of science. Something may be gathered of the possibilities thus opened up by reference to those of our illustrations which represent sections from animal and vegetable tissues as seen by the ordinary transmitted light and also by reflected light. It will be readily understood that in this way one can obtain a very much better idea of the three dimentional properties of the material under examination than by the ordinary method.

Microphotographic work of this kind is likely to shed much light on many disputed questions, for as yet there is a good deal of mystery and conflict of opinions surrounding some of the effects under consideration. At the same time, it must be said that the method is decidedly calculated for the use of the specialist, so that the number of competent observers working in the field is comparatively small.

The Robin Microphotographic Camera.

Cellular Animal Tissue Viewed by Transmitted Light.



Longitudinal Section of Steel Rail.

Cellular Animal Tissue, Oblique Illumination.

Printing Telegraphy

A System Which Readily Adapts Itself to Wireless Work and Secret Code

MR. A. M. HOVLAND, commander in the Norwegian navy, has devised an apparatus for sending secret messages by wireless telegraphy, which works as a printing telegraph, so that the messages are printed in the usual letters of the alphabet upon tape, but cannot be read by outsiders. Such a method is likely to be of value not only for army or navy use, but also in many other cases where secret messages are desired. In his new apparatus he uses a method by which any Morse letter can be set up in type at the receiving station by apparatus operated at the sending end. This type is then used to make electrical contacts so as to print this particular letter, as will be seen further on. In order to set up the type he divides a letter into different impulses of equal length, taking account also of the spacing, as shown in Fig. 1. Thus the letter J (on

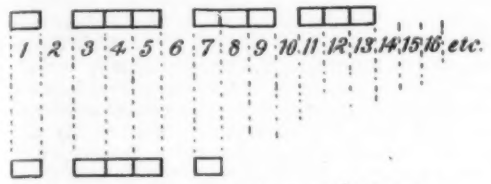


Fig. 1.—The Letter J Above and R Below.

the European system) consists of one dot and three dashes. Each dash is made equal to three dots in length, and the space is also equal to one dash. In a frame are placed type members which are provided with a tail projection (Fig. 3). Each type member represents one unit space, that is, to set up the letter J we push up on the projections so as to raise No. 1 (for the dot), leaving No. 2 lowered for the space, then raise Nos. 3, 4 and 5 for the first dash, and so on until we have the entire letter J set up in the frame.

The setting up is done from a distance by means of an electromagnet striker (Figs 2 and 3), which moves along upon a screw shaft rotated by an electric motor. At the sending end, electrical impulses are sent over the line (or by wireless) into the magnet at just the right time to raise the proper projections and omit the others, this being carried out in the usual way by a synchronous motor device at each end. The sending operator is thus able to set up the type for the letter J at the receiving station, and the next step is to have this letter printed on a paper strip as in the printing telegraph. The type member itself is not used for printing, but serves to operate the printing telegraph wheel which carries letters around it so as to come down upon the paper at the right time. This is done by using the type members raised as described above in order to make electric contacts. Above the type is a frame which carries rows of electric contacts, and each row represents a Morse letter. These letters are arranged according to their length, and when the type is moved over the rows, it finally comes upon one row in which it is able to bridge over all the gaps and thus send a current into an operating device. That is, when the type J comes upon the row J it will close the circuit, and this causes the movement of the type to stop in order to prevent it from passing over the remaining rows and making other contacts. When the contact is made, current passes into a printing wheel device so that the corresponding letter is printed upon the tape. The moving type piece is then restored to zero and can start over again for another letter.

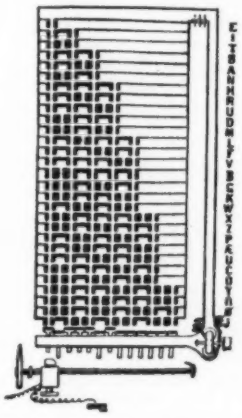


Fig. 2.—Frame of Contacts Corresponding to the Letters of the Alphabets.

The sending device as adapted for wireless telegraphy is seen in Fig. 4. It is designed so that striking one key of the keyboard such as A, will send out the right set of impulses so as to form the letter A in the receiving end. A contact arm *C* rotates above a set of eight contact segments. When key A is pressed, it makes circuits so that segments Nos. 1, 2, 3, 6, and 7 will be charged. When the contact arm rotates, the impulses will thus be sent out in this order by the relay device *f g*, and the wireless waves will be transmitted in the same way so as to act on the distant type-setting device, as we have already seen. Then the arm *C* returns to rest. In Fig. 6 is seen the receiving apparatus. Here the set of gaps is mounted on a drum *C* and the type composer is seen at *h*, with the moving electromagnetic striker *k*. This moves along under the type setter, and when the wireless apparatus receives current, the striker sets up the type. Instead of actual type, a set of raised contacts *i* is here used. The metal pieces and gaps lie on the cylinder *a b*, and on the same shaft is an inked type wheel *N*. Under the wheel runs the tape and a lever *g* worked by the magnet *e* presses the tape against the wheel to print a letter. It will be readily seen that as soon as the cylinder has rotated so that the type corresponding to letter A will bridge over the gaps in that row, current is thus sent into the magnet, the lever is raised and the letter A is printed on the tape, as this letter lies just in line with the row A. Given a system of this kind, it is now an easy matter to use a secret alphabet, for all that is needed is to shift the printing wheel about and set it so that when the letter A should be printed, we print D instead, and so on. The message is thus printed on the tape in other letters and cannot be read except by one who knows how much the printing wheel has been shifted. Another method is to use interchangeable contact strips upon the drum *a b* so that by changing these about, the printed message will be different and cannot be read by outsiders. We have omitted various details which are common to all apparatus using synchronous motors, and the inventor claims that all the difficulties of this nature have been overcome, and the experiments made over a telegraph line between Melsomvik and Tjömö were very satisfactory. The next step was to adapt the device for wireless working. Up to the present messages have been sent only over that distance. But there seems to be no obstacle to the use of the system over any distance within the limits of ordinary wireless telegraphy.

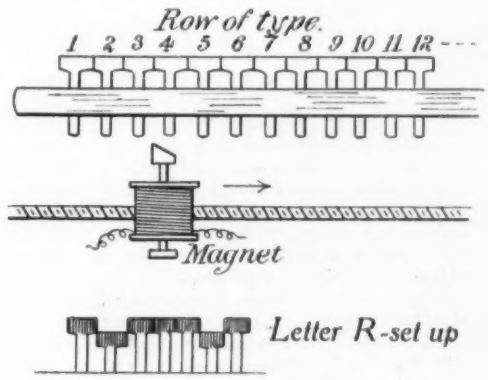


Fig. 3.—Diagram of Traveling Magnet and Type Members Depressed Thereby.

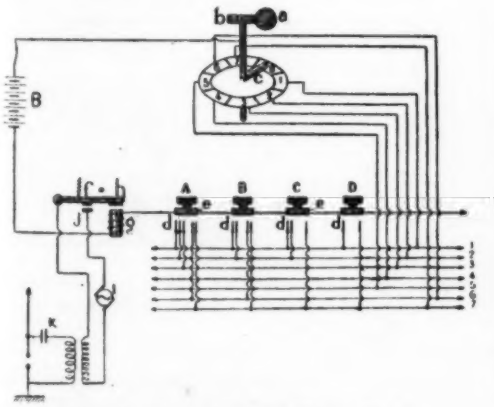


Fig. 4.—Diagram of the Transmitter.

Fig. 5.—General View of the Hovland Apparatus.

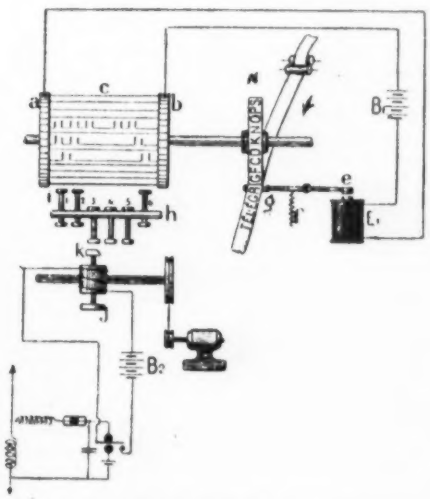


Fig. 6.—Diagram of the Receiver.

The Diffusion of Hydrogen Through Balloon Envelopes

The loss of hydrogen through the ordinary balloon envelope has long been attributed to the diffusion of the gas through the pores of the caoutchouc. There is, of course, not only an escape of hydrogen, but a simultaneous diffusion inward of air. The air which thus enters is, however, richer in oxygen than normal air. The exact condition of affairs is discussed in *Cosmos* by Mr. G. Oustervell, as follows: There is at first a loss of gas somewhat less than 10 liters per square meter per day, and as the inflating of the balloon proceeds, this number rises to 30, 50 and even 100 liters per square meter per day. If the balloon is then allowed to collapse, the permeability of the envelope gradually diminishes and finally returns to its original value of 10 liters per square meter per day. The cause of this effect is a reversible phenomenon which takes place within the tissue itself. It is, in fact, the absorption of hydrogen by the colloid matter of the caoutchouc or rubber tissue of the balloon. This, when placed in contact with hydrogen, absorbs the gas, and after a certain time the substance of the envelope is no longer simply the caoutchouc tissue, but a kind of combination of this with hydrogen, through which diffusion takes place with some readiness, and with increasing ease as the proportion of hydrogen in the envelope rises. And this is just what happens when the envelope is left for a long period in contact with the hydrogen, hence the increase in permeability of the envelope after inflation; when, on the other hand, the balloon is deflated, the envelope gradually gives up its hydrogen contents and thus resumes its comparatively impermeable quality. It will be seen, therefore, that caoutchouc is not an ideal substance for balloon envelopes.

Life Without Oxygen—III*

The Anaerobic Beginnings of Life

By Carl Snyder

Concluded from Supplement No. 1890, page 187.

Anaerobiosis and the Biogenetic Law.—We may now put the theory of anaerobic origins to what, in many minds at least, is a crucial test.

It seems to be fairly clear that every organism is in some sense an historical document and that in a broad way the development of every individual (ontogeny) reflects the history of its race (phylogeny). This is the recapitulation or "repetition" theory or biogenetic "law," in Haeckel's phrase, so long discussed, so much disputed, which now seems in process of being firmly grounded on bio-chemical experiment, free from the subjective idiosyncrasies which have so obviously begotten the acrimonious controversies which this question has aroused, although it is evident enough that we cannot regard it as a "law" in any rigid sense.

Likewise there are now few, even among biologists who doubt that the processes of growth have a chemical basis and that the structural and morphological differences between species, classes and orders are in the end the expression of definite chemical differences. This is clear enough from various considerations: from the fact that the hæmoglobin of different species crystallizes in different systems; from the production of a vast variety of distinct chemical substances, as for example, the organic acids, some of which are *characteristic* of a given species; and from many similar considerations, but above all from the wondrous development of blood- and serum-chemistry, revealing as this does the most curious affinities, antagonisms, differences and degrees of differences among the different types.

If this be true broadly, for racial characteristics, that is to say for evolution in general, it must be true in greater or less degree for each individual organism. The latter must present a corresponding chemical evolution through the different stages of its life; and for this there is a considerable and constantly increasing evidence. Just as, for example, certain of the vital enzymes appear only at certain definite stages of development, as the lab ferments in the second month of fœtal life in the human organism, diastase in the third month succeeding birth, etc., so in the ripened fruit are certain chemical substances which are not to be found in the leaf nor the stem nor the root nor the seed.

Upon all this we might base a reasonable expectation that if, in the evolutionary scale, we find an increasing need of oxygen corresponding to an increasing complexity of structural and chemical organization; if in other words primitive life is anaerobic and ordinary aerobic respiration represents an evolution from this fundamental form; and if, further, the recapitulation theory holds good, we shall find all this mirrored in individual development. The embryo will have less need relatively of oxygen than the adult and this need will increase parallel with the growth of the organism. What now are the facts?

W. Roux and Jacques Loeb were among the first to investigate this question. Roux found that the eggs investigated by him were incapable of development in the complete absence of oxygen and soon disintegrated. This result was confirmed in part by Loeb, who, however, found that among the eggs of various animals there is a wide disparity in this respect. Thus for example, the eggs of surface-swimming animals are very sensitive to the absence of oxygen, while in those whose density is sufficient to cause them to sink the oxygen need is enormously reduced. So, for example, the eggs of Fundulus continued to divide during twenty-four hours in an oxygen vacuum and during twelve hours in a current of hydrogen.

But what was more important still, Loeb established the fact that "the embryo is the more sensitive to a lack of oxygen the older it is." Thus an egg put into an oxygen vacuum twenty-four hours, thirty-six hours, etc., after fertilization was correspondingly less resistent to this withdrawal. So in turn the newly-born fish was still less resistent than the embryo (*l.c.*).

This general law established by Loeb, that the need of oxygen is inversely proportional to age, i. e., size, has been further extended and confirmed through the researches of H. Samassa, E. Godlewski and K. Amerling for animals and by W. Palladin and O. Nobokich especially for plants.

Samassa found and Godlewski in a series of researches confirmed the fact, that frogs' eggs continued to divide during twenty hours after fertilization in the complete absence of oxygen. Compare now this result with the experiments of Amerling on the larvæ of frogs at different periods of development. The latter took as his test the time which elapsed before the larvæ showed distinct signs of cessation of their normal activities (Lähmung). His results are shown in the following table:

Stage.	Body Length in MM.	Duration of Activity.
1	5	10 hours
2	6	7 "
3	7	8 "
4	8	6 "
5	11	2 " 15 min.
6	15	1 hour 10 "
7	20	1 " 0 "
8	24	55 "
9	32	52 "
10	34	40 "

In conclusion Amerling observes:

"While these experiments have been conducted on those stages in which the membrane has been shed, it is obvious that from the beginning of division to the stage of metamorphosis, that is during the entire larval life of the frog, sensitiveness toward the withdrawal of oxygen progressively increases."

These results find a clear explanation and the theory here under review a solid support, in the researches of A. Herlitzka on the ontogenesis of the enzymes in the germinating organism and especially the date of the appearance of the oxidases. It is well known since the classic work of Bourquelot, G. Bertrand and others that the oxidation of the organism are effected exclusively by means of oxidizing catalysts, the so-called oxidases. In the eggs of the viviparous animals and presumably in the unfertilized germ cells of the vivipares, these oxidases are apparently absent or in an inactive state. Says Herlitzka:

"It is only at a late stage in the course of development that the oxidases and peroxidases appear. The poverty of oxidative processes which is indicated by this lack of oxidising ferments finds confirmation, in the case of frogs, in the fact that in the embryos hæmoglobin is for a long time lacking in the circulating blood. The same is true of the permanent larvæ of the Congeracei (teleosts). For the rest, such a limited development of the oxidative processes was to be foreseen in organisms which undergo such rapid growth."

The same general conditions appear to obtain among plants. Thus, for example, Godlewski found that lupine seeds germinate and show a vigorous development in the complete absence of oxygen when an adequate supply of sugar is present. O. Nobokich showed that in Indian corn (*Zea Mays*) normal karyokinesis may continue during forty-three hours in the absence of oxygen.

Palladin sums up his researches thus:

"As to the capacity of plants in general to endure the withdrawal of oxygen (anaerobiosis) one may say—disregarding single instances of certain sharply defined cases—that the lower plants support anaerobiosis better than the higher and the embryonal or young stages better than the full grown."

From this wide series of researches on the most diverse organisms of both the animal and vegetable kingdoms, it is evident that we have here, first of all, one of the most admirable and best sustained proofs of the fundamental validity of the recapitulation theory or biogenetic law, free from the personal bias of investigators; and secondly a broad curve of continuity, in individual development, parallel to the same curve of continuity which obtains in racial development. Alike in the single organism, as in differing types, the need of oxygen is the result of and increases proportionally with increasing complexity of organization.

But apparently the simplest cell which we know is itself a highly complex symbiosis, colony or society of independent living units. In no other way can we account for the characteristics and life activities of the granules or plastids which are seen growing and dividing in the living cell and whose division is apparently precedent to the division of the cell itself.

Let us now prolong this curve of decreasing oxygen need with decreasing complexity and it is apparent that we soon reach a point where this need approaches zero. Have we any right to assume that this curve is an asymptote? Clearly the demonstrated existence of absolutely anaerobic organisms is evidence that we have not.

Doubtless it would be a little hazardous to describe all meta-cellular organisms simply as congeries or colonies of anaerobic plastids, granules, biophores or "protomeres." And yet many facts are in favor of such a conception. If it can be established and such is the present drift, we shall then have a clear, simple, mechanical explanation of what is now wrapped in fog.

In any event, this much is sure: at the foot of the life-scale, alike in the simplest of organisms and in the embryonic conditions of all organisms, the need of oxygen is wholly lacking or very slight.

This must have been true historically, if evolution be true, else we shall for ever remain without the remotest clue to the actual mode of the beginning of life, either in this or in any other regard, for alone in the fact or conception that the living world and each individual organism is, like the earth itself, a mirror of its own history, is any kind of clue to be found. We might manufacture life in the laboratory, as in the laboratory we may, for example, make coal. But in neither the one instance nor in the other would there be any necessary or positive indication of their natural origin. Here biology and geology, biogeny and geogeny, are at one, for every direct trace of the early history of life, like that of the primitive history of the earth, has been utterly and for ever destroyed.

Yet geology has its word on the special problem here under consideration. There is oxygen in the present atmosphere: was there any in eozoic time?

The Atmosphere of the Primitive Earth.—When we consider the oxygen of the atmosphere in present and in former times, we meet with the following facts:

In the lower levels of the existing atmosphere oxygen forms a little less than one-fourth of the air, by weight, the amount decreasing as we ascend, so that in the upper levels of the atmosphere there is no oxygen at all. Even after including the amount dissolved in sea water, the quantity of uncombined oxygen as compared with the combined oxygen of water, for example, it is almost infinitesimal. It is computed that the waters of the ocean, evenly distributed over the globe, would form a spherical shell about 2,500 meters thick. This would represent an average pressure of 250 "atmospheres," and as eight-ninths, by weight, of this water is oxygen, it follows that this would be easily 1,000 or 1,200 times more than the uncombined oxygen contained in the atmosphere, the ocean, etc.

We have next the enormous quantity of oxygen locked up in the earth's crust. The average thickness of the sedimentary rocks exposed to view has been variously computed at from 10,000 to 50,000 meters. Let us for the argument take the lower of these estimates; the relative density of the rocks is approximately 2.5 and they are about one-half composed of oxygen. The total amount then is at least five times the amount in the sea, it may readily be ten or twenty times. It follows, therefore, that the amount of atmospheric oxygen is certainly not a 1/5,000, possibly not a 1/25,000 part, of the total amount in the outer envelope of the earth. As to the amount in the interior of the earth we of course know nothing whatever.

It is very well known that large quantities of oxygen are withdrawn from the air by the oxidation of the materials of the rocks, the respiration of animals, etc., and that there is practically but a single source of return supply, that of plant activity. We may then regard the oxygen in the existing atmosphere in one of three ways:

1. As the residue of an enormously greater quantity in former times.
2. As liberated under the influence of internal heat, volcanic eruptions and the like from that occluded in the rocks.
3. As due entirely to plant action.

1. So far as the first supposition is concerned, we are met by the fact that throughout the entire lithosphere or rocky crust are masses of oxidizable but unoxidized materials, large areas of which must have been exposed at one time or another to atmospheric action. This fact, familiar since the days of Daubeny, is not compatible with the view of a previous great excess of oxygen.

Further than this, it would seem that in former geologic times, rock weathering and oxidation must have gone on rather more intensely and on a wider scale that at present. It is now increasingly evident that in the earlier ages at least the land areas were devoid of vegetation.

The continents appear to have been utter deserts, such as are now unknown. We have clear evidence of the existence of an atmosphere and likewise of climatic zones, much as at the present day; the average temperature was probably much the same as now. These desert conditions would seem to indicate much greater

* Reprinted from *Science Progress*

and more frequent precipitation and far more violent winds and storms. Æolian erosion would have been far more prevalent and active than now and the plantless continents would have been swept by enormous dust storms. Under these conditions, oxidation would have gone on at a very intense rate; so that if, as some writers suppose, a greater excess of oxygen existed in the beginning, this would have been quickly and completely exhausted. We may therefore dismiss the first possibility.

2. Within the last few years, thanks to the researches of Moissan, Gautier and A. Brun, we have learned something of volcanic chemistry and it may be taken that practically no uncombined oxygen is present in volcanic emanations. On the other hand, we know of vast quantities of hydrogen compounds and there is the probability of free hydrogen, so that we have here an agency working for depletion of oxygen rather than the reverse. And plutonic action is probably only volcanic action on a very grand scale. For the rest there seems to be no oxygen free in meteors; and there is no other visible source of supply. We may therefore dismiss the second possibility.

3. It is a curious fact that we should be led to the third conclusion directly from consideration of a very simple fact. Taking the amount of coal estimated to be under the best known areas, Great Britain for example, if we suppose that this represents the average distribution for all the continental and "transitional" (i. e., shallow water) levels of the earth, we arrive at a quantity of carbon which would be approximately the chemical equivalent of the amount of oxygen now in the atmosphere. We know that coal is the product of the great water-plants which flourished at a maximum in the so-called Carboniferous period; in other words, it was due to the splitting up of carbon dioxide under leaf action. And it is supposed, as other considerations suggest, that in subsequent times the give and take of oxygen to and from the atmosphere has been maintained at something like the present balance. Since we know of no other adequate source of oxygen, it seems fair to conclude, therefore, that antecedent to plant life the atmosphere was practically devoid of this element.

This view of an oxygenless atmosphere if former times was put forth as early as 1845 by C. J. Koene of Brussels. It has been supported in later times chiefly by T. L. Phipson, J. Lemberg, John Stevenson, Lord Kelvin and Arrhenius, it would seem to be a necessary postulate of any meteoritic theory of earth origins, since, as noted, gaseous oxygen does not appear to be a constituent of meteorites. A radically opposite view, of a former enormous excess of oxygen, developed among others by H. Lenicque, is due to exclusive consideration of the chemical side of the problem, without regard to the known geological facts. It is incompatible with the existence of oxidizable but unoxidized compounds, present in enormous quantities, especially and particularly in the igneous rocks. As already indicated, this would involve a very rapid absorption of oxygen as soon as magmatic extrusions were exposed to atmospheric action.

These considerations establish with a fair degree of probability, first that the oxygen in the present atmosphere has been derived largely if not exclusively from plant action; secondly that life began in the presence of very little oxygen, if the latter was not absolutely lacking. This conclusion derives further support from the history of the mechanism by which, in the above view, the oxygen we breathe has been set free.

It is well known that the characteristic plant function, the splitting up of carbon dioxide accompanied by the liberation of oxygen, is due to the presence of a complex substance, chlorophyll, whose function apparently is to absorb the lower rays of the spectrum and with this energy to dissociate the molecules of carbon dioxide and water, and these according to the prevailing theory gave rise to formaldehyde, CH_2O, oxygen being liberated. These "color bodies" or chloroplasts present one of the most interesting instances of transition from highly organized to living substance. There is no evidence, as yet, that they can live by themselves; on the other hand they divide and reproduce in exactly the same manner as unicellular animals and plants.

We seem to have here the clearest evidence of a symbiosis; community life, we know, extends very far down the scale even to the green algæ and their like. A step lower in the scale and this association with the complicated chlorophyll mechanism is lost. We have then the series of bacteria and molds which, apparently, do not utilize carbonic acid and often give off hydrogen instead of oxygen.

It is to be noted further that in the metaphyta and most strikingly in the higher plants, the chlorophyll does not exist obviously in the seed and does not appear until long after germination has begun. We should infer from this that the chlorophyll "habit" was of relatively late appearance in the plant world and this conclusion is precisely that to which we are led from consideration of the non-chlorophyllian autotropic organisms. In other words, we have no reason whatever for supposing that the chlorophyllian plants are anywhere near the bottom of the life scale.

The most reasonable supposition that we can make is that the liberation of oxygen in great quantities by plants began long after a teeming life filled the seas and had at least begun the conquest (i. e., occupation) of the soil. We have no reason, however, to suppose that the great bulk of the oxygen of the atmosphere was due to land plants. On the contrary, we have the clearest evidence, from the characteristic "color zones," that climatic conditions were not essentially different far back in the beginnings of the Cambrian and probably antecedent to the Algonkian. It is possible that the chief producers of atmospheric oxygen were the green algæ of the sea and they may be still. Phipson reports that the green algæ produce, weight for weight, fifty times the quantity of oxygen liberated from a common phanerogam, Polygonum.

To sum up: from every consideration affording a clue to the conditions under which life might have arisen naturally on the earth; from the study of the comparative biology of plant and animal and the protista as well as the embryological, developmental, physiological and bio-chemical processes; finally from such geological evidence as we possess, the one conclusion emerges: that the beginnings of life did not involve the intervention of uncombined oxygen.

The gain from this new point of view may not be great, but it enables us to *delimit* the problem one step further, so that we may now say, in the light of our present knowledge: that since anaerobiosis and the fermentative processes are identical, the problem of the genesis of terrestrial life has become that of the natural origin of the organic fermentative substances or enzymes, whose simple collocation into plastids, protomeres, biophores or granules constitutes the primary living units. The aggregation of these latter in turn constitutes the "cell."

The Chemistry of Raw Sugar Production—II*

By Charles A. Browne

Concluded from Supplement No. 1890, page 183

THERE is a great difference in the physical properties of raw cane sugar and raw beet sugar. Raw cane sugar has usually a pleasant, fragrant odor and a delightful taste which many prefer to the refined product; this is due to the presence of invert sugar and the slight caramelization which this invert sugar has undergone during manufacture Raw beet sugar, on the other hand, has a very objectionable odor, due to the nitrogenous substances of the beet root and a very bitter taste due to the mineral salts taken up by the beet root from the soil. Beet sugar is not a fit article of food until it has been refined. Attempts have been made to render raw beet products more palatable by subjecting them to a slight inversion and caramelization but this method has been only partially successful.

Many processes have been devised for recovering the sugar contained in low grade beet molasses. In the short time at our disposal we must pass over the osmose process, barium process and other methods of desaccharification and describe briefly the saccharate process of Steffens. In this process the molasses is diluted with water to about 12 to 14 per cent solids and the solution treated in the cold with very finely powdered lime, using all the way from 80 to 150 parts of lime to 100 parts of sugar. Constant agitation of the solution is necessary to prevent the formation of clumps of lime and care must be taken to keep the temperature down. The insoluble tri-saccharate of lime which is formed is filtered in presses and washed with cold water to remove impurities. This tri-saccharate is then mixed with hot washings from the filter presses and added directly to the diffusion juice for saturating; the saccharate is decomposed by the CO^2 and the liberated sucrose worked up with that of the juice. In some factories the saccharate sugar is treated separately; there are many modifications of this process and every factory has a method of its own.

A peculiar impurity found in sugar beet products is the sugar raffinose, which has a polarizing power over 50 per cent greater than sucrose. A very small percentage of raffinose will introduce serious errors in the control of factory operations and the chemist may wonder why his yield of crystallizable sugar does not come up to his polarizations. The sugar refiners in England have been much agitated the past year by finding large amounts of raffinose in raw beet sugars from Germany and a congress was held in Berlin to arbitrate the question.

In addition to raffinose there are gums and pectinous substances in the beet which affect the polarization and introduce errors in the calculation of the sucrose balance. Similar gums are also found in cane products. Every well-organized cane sugar and beet sugar factory maintains a strict account of all the sugar which enters the factory in the cane or beet and all the sugar which leaves the factory in raw sugar or molasses. The difference between the two should equal the determined losses of manufacture, such as sugar lost in the press cake, sugar lost in the spent beet chips, etc. If there are any undetermined losses the chemist is called upon to explain; the papers and discussions upon undetermined losses would alone fill several volumes every year.

In the present condition of the sugar industry the chemist has to understand not simply the chemistry of sugar; he must also be perfectly familiar with the chemical properties and behavior of all the non-sugars which do and may occur in his raw products. A German chemist, Rümpler, has written a book upon the non-sugars of the beet; the list of compounds runs up into the hundreds. Every technical sugar chemist should master the contents of this book. It is a pity that no such work has been written upon the non-sugars of the cane.

The application of strict scientific principles has been followed more energetically in beet sugar manufacture than in cane sugar manufacture and this we would expect for the beet industry was developed in the highly civilized European countries where skilled labor was abundant and the cane industry was maintained in primitive tropical countries where labor was shiftless and unskilled. This condition of affairs existed for many years and gave beet sugar for over two decades a strong ascendancy over the cane. In 1852, taking the whole supply of the world, there was six times as much cane sugar manufactured as beet sugar, beet sugar kept gaining, however, until 1884 the production of the two kinds of sugar was about equal, a little over two and one-half million tons each. Beet sugar then took the lead until in 1899 the production of beet sugar was about five and one-half million tons and that of cane about three millions. Then came the ending of the Cuban war and with it the introduction of the scientific principles worked out for the beet industry to the old dilapidated cane factories of Cuba and Porto Rico. Modern methods of manufacture had also been busy reorganizing the cane industry in Hawaii, Java, Australia, and other countries. The production of beet sugar in 1901 reached nearly seven millions tons and since then has remained about stationary; but the increase in cane sugar productions has gone forward each year by leaps and bounds. In 1907 the cane had caught up to the beet, the production being a little over seven million tons apiece. In 1908 cane sugar was over 600,000 tons ahead and in 1909 nearly 2,000,000 tons ahead out of a total world's production of about 15,000,000 tons. This supremacy the cane seems destined to maintain.

There are several reasons for the belief that the future development of the sugar industry will be more along the cane lines. There is first the cheaper cost of producing cane sugar; the average cost of producing one pound of raw sugar being about 1.75 cents for the cane and 2.25 cents for the beet, a difference of 0.5 cent in favor of the cane. There is second, the much greater yield of sugar per acre which the cane gives over the beet. Under the best systems of cultivation and manufacture such as are found in Hawaii and Java, over four tons of sugar can be produced per acre. Germany, which leads in the production of beet sugar and in the yield of this per acre, can raise barely two tons of sugar per acre. Then there is third, the preference which the refineries of England give to cane over beet sugar. Yet notwithstanding these advantages possessed by the cane industry, the production of beet sugar seems destined to hold its own for many years to come; and indications are that the present year will see a record-breaking beet sugar crop of 8,500,000 tons. The consumption of beet sugar, however, will be limited largely to those countries where it is produced; it cannot compete with cane sugar in foreign markets. At the present time the consumption in the United States is about evenly divided between beet and cane sugar.

* Republished from the *School of Mines, Quarterly.*

The Arch Ring on Removal of the Falsework.

The Runway for Hoisting the Back-fill.

A Concrete Bridge of Striking Design

A Pleasing Adaptation of Old Mission Architecture

By Frank C. Perkins

QUITE in keeping with the historic associations of the locality is the reinforced concrete bridge recently completed at Piedmont, California. Although built according to the most modern engineering practice, it preserves the simplicity and quaintness of the old mission type of architecture. An attractive novel feature of the bridge is the introduction of four covered resting places supported by light concrete columns, covered with Spanish S tile

At the portals are four pylons at either end, capped with a decoration cast in concrete and inclosing large ornamental electric lights. The total length of the structure is 362 feet 10 inches, including a central arch span of 130 feet with symmetrical approaches composed of reinforced slabs supported by girders and columns inclosed in 6-inch exterior curtain walls. Each transverse bent is made up of three columns.

The roadway is 22 feet wide, with an additional 6-foot sidewalk on either side. The intrados curve of the arch was chosen for beauty as well as economy. Both temperature and rib shortening were provided for. The slab of the approaches and the arch were designed to carry an interurban electric car, and the sidewalks were designed for a live load of 150 pounds per square foot, carried by ornamental brackets rigidly connected to the arch rib and approach slabs.

There was a timber trestle on the site with about 40,000 feet of lumber in the trestle, largely used for shoring and centering for the new bridge. The falsework of the arch was made of 8 by 10-inch, 8 by 12-inch and 10 by 12-inch stock. The bents were on approximately 5-foot centers, and the posts of each bent on 6-foot centers. The posts were placed normal to the intrados curve, and were supported on continuous sills firmly embedded in the soil. The falsework was unusually heavy and clumsy, but the timber from the trestle was old and its strength could only be guessed at. Heavy caps were placed transversely on each bent, the caps in turn supporting longitudinal strings on 2-foot centers. On the stringers 2 by 4-inch pieces were blocked up and sprung to the curve. In some portions of the intrados curve where the curvature is great, 2 by 4-inch stock could not be sprung without splitting, and use was made of two 1 by 6-inch pieces nailed together. These readily took the desired spring, and the variation from the true curve was practically nil when the decking was finally placed. Constant wetting down (sometimes four times daily) kept the decking in excellent condition. Points on the intrados curve, near the crown, were built ¼ inch higher than required to allow for settlement in the foundation of the falsework.

The bridge was designed by John B. Leonard of San Francisco, and the writer is indebted to Engineer W. P. Day of San Francisco for the photographs and data. The concrete of the rib was poured through timber chutes leading from a hoist at the center. Doors in the chute were provided in case the concrete flowed too rapidly, and separated the coarser from the finer components, but their use was found unnecessary, the pitch of the chute being a minimum.

On the completion of the casting, and for several days thereafter, the arch was wet down four times daily. The usual buckling of the forms at the crown after pouring the haunches did not occur owing to the inclination of the posts. The falsework remained under the rib for about six weeks. It was deemed best to strike centers before the spandrel wall forms were set in place, so as to allow the arch to take its unconstrained shape, without subjecting the walls to the consequent stress.

The approaches are symmetrical, each being a 15-inch reinforced slab supported on transverse girders. The entire lower approach was poured through a chute, the length of the latter from the hoist over the mixer being 180 feet. The chute was 18 inches wide and 10 inches deep, and has a drop of 2½ inches per foot.

The mix was run a little wet and produced a perfect concrete at the extreme end. On account of the depth of the girders they were poured in two parts, 4 by 12-inch blocks, and ⅝-inch rods being used to provide for horizontal shear at the plane where the work was stopped. All of the side walls were poured to the bottom of the sidewalk. The upper approach could not be poured by the chute without building a very high tower, on account of the grade of the bridge, and resort was therefore had to the regulation buggies hoisting first from the mixer to a runway over the arch of the bridge.

Four expansion joints were used in the structure, each arranged so as to be hidden by the piers. The use of asphalt was originally contemplated, but was replaced by two thicknesses of heavy felt. Little or no dirt can penetrate the joint, and it is believed that a better working joint has been obtained than would have been produced with asphalt, as the latter would expand and contract with variations in temperature. About 2,250 cubic yards of concrete and 80 tons of steel were placed in this bridge.

The Optical Properties of Metals

A Study from the Standpoints of Theory and Experiment

By Prof. L. R. Ingersoll, University of Wisconsin

WITHIN the last two or three decades numerous experiments have developed the fact that, from the standpoint of physical optics, the metals as a class are of peculiar interest. Because of the enormous absorption they exert on transmitted light they long defied study in this field, but we now know that their optical characteristics are by no means limited merely to high reflecting and absorbing powers, but include also many of the more complex effects depending on the penetration of light. The metals indeed are found to exhibit not only refraction, dispersion, selective absorption and other phenomena which we are accustomed to associate with fairly transparent substances, but certain of this class such as silver and sodium stand out uniquely among all other substances, for they transmit light with a velocity many times greater than that in vacuous space.

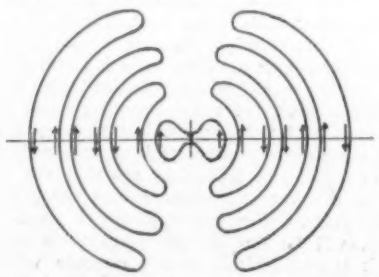


Fig. 1.—Diagram of the Lines of Force Near an Oscillator.

As a field for research metal optics has attracted a considerable number of investigators and substantial contributions to our knowledge in this line are made every year. Without attempting in any way a rigorous discussion, it is the purpose of this paper to outline the facts, as we know them to-day, of this field, which is one of the less widely known, but nevertheless extremely interesting branches of physics, and so far as may be to suggest the theory which has been evolved to account for them.

The Electromagnetic Theory of Light.—Before taking up in detail these optical properties it may be well to review briefly the salient facts of the theory which underlies all optical phenomena, that is, the electromagnetic theory of light. This was the product of the genius of the English physicist, James Clerk-Maxwell, and when we look at the long line of consequences, the new basis for optical and electrical theory, and the practical applications—of which wireless telegraphy is only one—we cannot but feel that he was more than justified in expressing his satisfaction to a friend as he did in a letter of 1865:

"I have also a paper afloat with an electro-magnetic theory of light, which, till I am convinced to the contrary, I hold to be great guns."

Reduced to its simplest dimensions this theory as put forth by Maxwell and experimentally demonstrated by Hertz and his successors, states that any oscillating current of electricity, particularly such as is produced by the discharge of a condenser, gives rise to a series of electro-magnetic waves which are propagated out in space with a velocity equal to that of light. The nature of such a wave as well as the reason for calling it *electro-magnetic* in character demands a little ex-

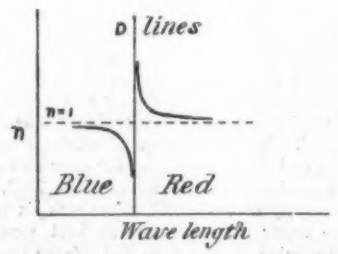


Fig. 2.—Dispersion Curve of Sodium Vapor.

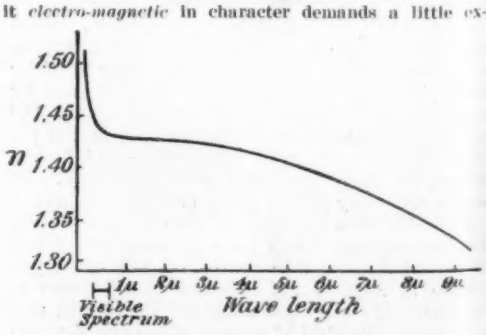


Fig. 3.—Dispersion Curve of Fluorite.

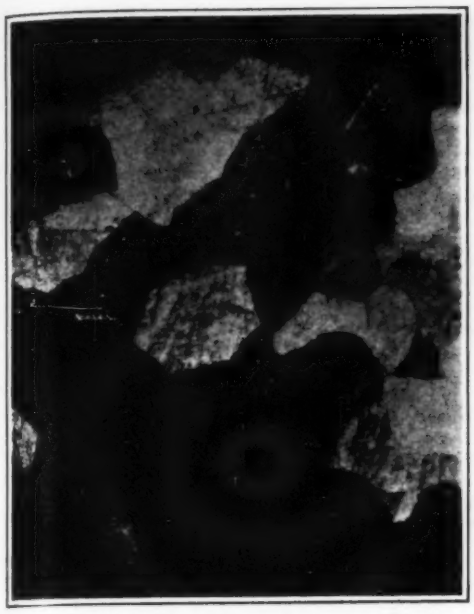


Fig. 4.—A Sample of Very Pure Iron, Annealed at 1,300 Deg. Cent., Magnified 50 Diameters.

planation. Suppose two adjoining conductors, such as the knobs of an electrical machine, to be suddenly given equal and opposite electrical charges. There is then a state of electrical stress, or in common parlance, *lines of force*, between these conductors, which did not exist before. This state of stress is not confined to the region just between the conductors, but spreads out like a wave to all parts of the surrounding medium. If now these electrical charges are suddenly interchanged, a stress in the opposite direction results, and if they oscillate repeatedly a train of waves is sent out, the lines of force being somewhat as represented in Fig. 1. Such oscillation is brought about by the passage of a spark between the conductors, for a single spark is found to result in a large number of back and forth surges of electrical charge, each of which takes place in a perfectly definite, though very short, period of time. This results in a constant *wave length*, or distance between successive pulses or waves, and this wave length will be proportional, both to this period and to the velocity of propagation.

But the electrical stress is not the only feature of such a wave, for it is accompanied in every case by a magnetic field. At the conductor the electrical stress causes a spark or current which gives rise to surrounding lines of magnetic force, while in the wave in space the analogue of this current is the so-called "displacement current," which results whenever the electrical stress in a non-conducting medium is varied, and which can also give rise to a magnetic field. To sum up then we have in an electro-magnetic wave rapidly varying magnetic and electric fields perpendicular to each other, while the wave advances in a direction at right angles to both.

We are now in a position to understand what is meant by a light wave, for the description just given of an electro-magnetic wave applies not only to "wireless" waves, which we are accustomed to think of as typical, but also to waves of light or radiant heat. The only difference indeed between these apparently diverse phenomena is one of wave length. From the wireless wave of several kilometers length down to the smallest electrical waves, measuring only about 6 millimeters in lengths, produced in the experiments of Lebedew and others, it is possible to get a continuously graded series of wave lengths. But below this there is a gap in the series until we come to the longest heat waves recently isolated by Rubens and Wood, which measure between .1 millimeter and .2 millimeter, after which the series is practically continuous down to the wave length of ordinary light, which lies between .0007 millimeter and .0004 millimeter, and even as far as the shortest ultra-violet wave lengths yet found which are no more than .0001 millimeter in length. The gap between the shortest electrical and longest infra-red radiation waves exists only because of our inability as yet to isolate longer heat waves or shorter electrical ones. The difficulty in the latter case will be appreciated when it is explained that the oscillator for 6 millimeter waves is made of minute pieces of fine wire.

The very small size of the oscillator which gives rise to these short electrical waves leads us at once to the conclusion that the vibrator which gives rise to light waves ten thousand times shorter is probably of atomic or sub-atomic dimensions. There is good reason for believing that the vibrator itself is that part of the atom known as the electron. This, which is nearly a thousand times smaller than the hydrogen atom, carries—or perhaps itself *is*—a charge of negative electricity, and by its oscillations inside the atom gives rise to light waves. But whether the radiations be produced by such an oscillator or start from the rods of a huge electrical machine, it is well to remember that they differ only in the matter of wave length, and indeed it is possible to reproduce practically all optical phenomena with electro-magnetic wave tens of thousands of times longer than those of light.

Fig. 6.—Optical Pyrometer Used in Measuring the Temperature of Metals by the "V" Method.

Transparent Substances.—The optical theory of transparent bodies is relatively simple and must be mentioned briefly before taking up the more complicated case of the metals. Such substances are always poor conductors of electricity compared with metals, so electro-magnetic waves will be propagated through them much as through the free ether, save in the matter of altered velocity.

Now the general expression for the propagation of any wave disturbance is

$$\text{Velocity} = \sqrt{\frac{\text{Elasticity}}{\text{Density}}}$$

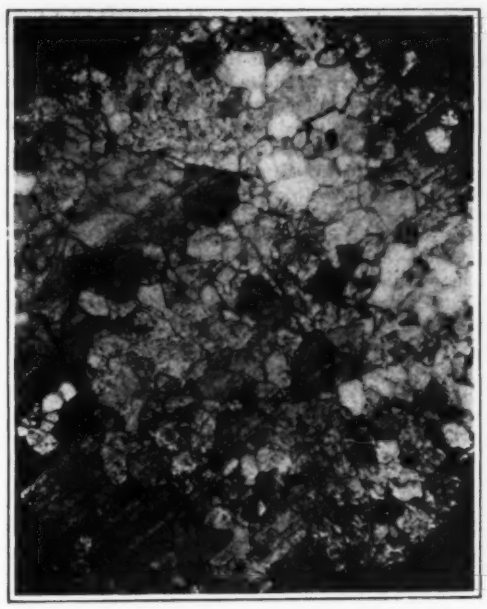


Fig. 5.—A Sample of the Same Iron Annealed at 1,000 Deg Cent., Same Magnification.

and this may be applied to the electrical case if we represent the so-called "electrical elasticity," as is sometimes done, by $\frac{4\pi}{K}$, and the corresponding density by $4\pi\mu$, where K and μ are respectively the dielectric constant and permeability of the medium. The velocity of an electro-magnetic wave in such a medium would then be proportional to $\frac{1}{\sqrt{K\mu}}$, or, since it is known that for such extremely rapid vibrations the permeability of all substances is unity, proportional to simply $\frac{1}{\sqrt{K}}$. $\sqrt{K}$ then represents the ratio of the velocity of light in free ether to that in the transparent medium, and since this ratio is nothing but the refractive index of the medium we have at once Maxwell's fundamental law of the electro-magnetic theory, that the square of the refractive index of a substance should equal its dielectric constant.

Unfortunately for the simplicity of optical theory this law is by no means generally obeyed. It is true that in some simple substances the agreement is excellent, for air gives values for the dielectric constant and square of the refractive index of 1.000590 and 1.000588, respectively, and carbon bisulphide 2.65 and 2.69, but most transparent media yield figures more like those for alcohol which are 25.0 and 1.85, respectively, or water, where the disagreement is even worse. Far from being fatal, however, this disagreement only strengthens our theory as it is susceptible of a simple explanation. As is well known, the refractive index of all substances varies with the wave length or frequency, and it is found that when the dielectric constant and refractive index are measured for as nearly the same frequencies as possible the above agreement is improved.

In explaining this change of refractive index, and hence of velocity, with frequency we are forced to the assumption that transparent substances contain large numbers of electrons which vibrate freely about their mean positions with definite periods. These are

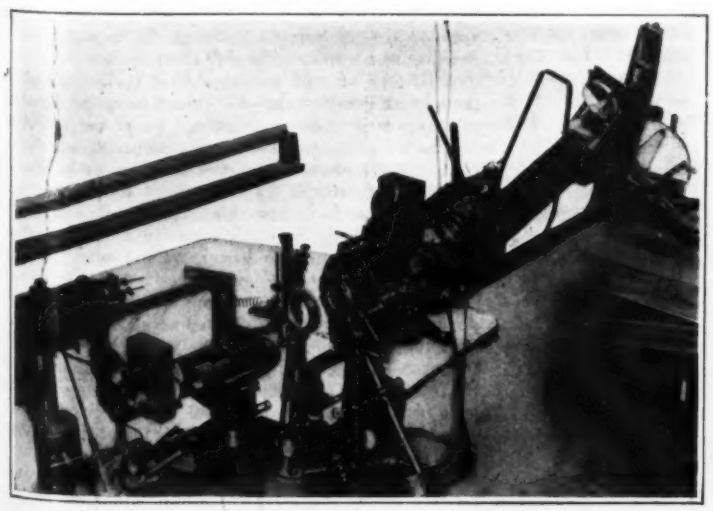


Fig. 7.—Apparatus for Measuring the Infra-red Dispersion of Metals.

Fig. 8.—The Optical Pyrometer in Use.

set in motion when the medium is traversed by a light wave, and if the frequency of the wave happens to be nearly the same as that of the electrons, it is evident that a much greater amplitude of vibration will result. This will give rise to a greater absorption of energy and also to a change in the velocity of propagation, for the same reason that the speed of a wave down a stretched cord will be altered by loading the string with small balls at frequent intervals. Such selective absorption—it is called *selective* because it has a large value only for frequencies nearly those of the freely vibrating electrons—is accordingly accompanied by a change in the refractive index, which is found to have a much higher value than normal on the longer wave length side of the absorption region, and lower on the shorter side.

This effect is best considered by studying the dispersion curves of some substances, for these give at a glance the relation between refractive index and wave length or frequency. Two such curves are shown in Figs. 2 and 3. The first is for sodium vapor, which has a great absorption for light of the wave length of the *D* lines, and the second is for fluorite, a typical transparent medium much like glass or rock salt in this result. It exhibits what might be called the normal dispersion of most substances, i. e., a slow fall of the index with increasing wave length, the slope of the curve becoming less steep at the same time. But it is to be noted also that after a certain region in the infra-red has been reached the curve begins to fall more steeply. This is supposed to be due to electrons whose natural periods come far out in the infra-red, but whose influence extends back to this point. We shall have more to say about such electrons later.

Metals.—We are now ready after this rather long but necessary introduction, to consider the optical properties of the metals themselves. The first and most striking difference between the transparent media we have been considering and this class of substances is the vastly greater electrical conductivity of the latter. This accounts for that most noticeable feature of all metals, viz., their high reflecting power; for when an electro-magnetic wave reaches a metallic surface the electrical stress cannot be sustained as it is in an insulator, but will give rise in the surface of the metal to a current which oscillates with the wave. This surface will then act in effect like a new radiator and will return a wave quite similar in every respect to the one reaching it, or in other words it will *reflect* the incident wave.

The phenomena of reflection may be studied to advantage with the longer electro-magnetic waves, using plane sheets of metal as reflectors. If these sheets are separated by cuts into narrow parallel strips an interesting fact is noted; for if the oscillator as well as the cuts are vertical so that the lines of electrical stress are parallel to the strips, the reflecting power is almost as good as if the sheet were solid, but if the oscillator is at right angles to this direction the reflection is greatly reduced. This is easily accounted for when it is remembered that the conduction currents which are generated can easily run along the strips in the first case, while in the second they cannot cross the cuts.

If metals were perfectly conducting their reflecting powers would be 100 per cent, but because they all offer a certain amount of resistance, the current which flows will not at once neutralize the electric intensity of the incident wave, so some portion of the latter will travel into the conductor. This transmitted part will proceed only a little distance, however, before its electric intensity is neutralized by conduction currents and the energy of the wave "absorbed." Theory points out that the so-called *optical constants*, n the refractive index, and x the absorption index, of the metal, are related to c the conductivity (in electrostatic units) and to T the period of vibration of the incident light, by the equation

$$n^2 x = c\,T$$

It has been found by Hagen and Rubens that this formula is so well obeyed for long wave lengths in the infra-red that the reflecting power, which is intimately related to the constants n and x, may be computed from the electrical conductivity, by the formula

$$R = 1 - \frac{2}{\sqrt{cT}}$$

Experimental.—Most of the efforts spent in studying the optical properties of metals have been directed toward the determination of the constants n and x already mentioned, for when these are known the other optical characteristics may be at once determined by computation. The most notable experiments in this line are those of Kundt, performed some thirty years ago. He succeeded in actually making a prism of metal so thin and of such a small angle that sufficient light would be transmitted for measurement. This light would suffer a very small deviation—a matter of a few seconds of arc only—but if this deviation and the angle of the prism could both be determined, the index of refraction could be computed just as for a transparent substance. By electrolytic deposition prisms of silver, gold, iron, and several other metals with faces of a few square millimeters were prepared, so thin that the prism angle measured only about 30 inches, while the deviation produced was even less than this in most cases. This deviation, however, could be measured, and it allowed a determination of the index of refraction with an accuracy of perhaps 10 or 20 per cent.

It was in these experiments that the striking fact was first noted that gold, silver, and copper deviated the light, not, as might be expected, toward the base of the prism, but *toward the edge*. This could only mean a refractive index less than unity, or a velocity of light greater than that in free space, and this placed these metals in contra distinction in this respect to all other known substances. That these results are correct, however, is shown by their having been checked many times since by other and more accurate methods which will now be considered. The conclusions may be anticipated sufficiently to say that not only have Kundt's measurements of these abnormally low refractive indices been found to be substantially correct, but one or two other metals have also been placed in this class.

These more accurate methods are based on the characteristic action which a metal surface exerts on polarized light, and owe their development largely to Drude. If plane polarized light of a certain azimuth is reflected at a certain angle of incidence from a polished metal surface, it is reduced to light of circular polarization; or conversely, circular light is reduced to plane at these angles. These angles of incidence and azimuth are known as the *principal incidence* ϕ, and *principal azimuth* ψ, respectively, and the theory shows that the optical constants n and x are related to these angles by the simple equations:

$$x = \tan 2\psi, \text{ and } n = \frac{\sin\phi \tan\phi}{\sqrt{1+x^2}}$$

The process of determining the optical constants of any metal involves first the securing of a very perfectly polished surface of the metal, and second the measurement of these angles of principal incidence and azimuth. The first task is by all odds the hardest, and in some cases indeed well nigh impossible of accomplishment. It is difficult enough to put a good polish on a metal surface, free from pits and scratches and from surface films, but when there is added to these requirements the one which is demanded in many cases, viz., that the surface shall also be optically plane, the difficulties are such as can be overcome by only a very few optical workers in the world. Fortunately, in many cases less rigorous requirements of planeness will suffice, and under these circumstances the surfaces can be prepared without a great deal of trouble by grinding and polishing with graded carborundum and water on a flat revolving disk, preferably covered with cloth. The finishing is done with rouge or Vienna lime; but in many cases, particularly with the softer metals, any abrasive, whether it be carborundum or rouge, will stick in the metal, so the surfaces have to be prepared by careful filing and "burnishing" with a polished steel tool. The quality of surface obtained depends not only on the process employed, but also to a certain extent on the previous treatment of the specimen. Surfaces frequently show a crystalline structure which is detrimental to measurement, and this structure is found to depend on the degree of heating to which the metal has been subjected. Figs. 4 and 5 show the crystals in a very pure iron, magnified about fifty diameters. The smaller crystals were formed by annealing at a temperature of about 1,000 deg. Cent. and the larger at 1,300 deg. Cent.

For measuring the principal angles some type of optical polarimeter is necessary. A large spectrometer, with the prism removed, fitted with two Nicol prisms and a Babinet compensator is frequently used for this purpose. While the compensator furnishes a very good means of detecting or analyzing elliptically or circularly polarized light, the writer has made use of a scheme whereby circular light is recognized by the fact that it appears the same under all azimuths, which method has certain advantages of simplicity and usefulness for a larger spectral range.

The optical constants for a number of metals for the wave length of sodium light are given in the following table:

TABLE I.

Metal—	n	x	nx	R (reflecting power)
Steel	2.41	1.38	3.40	58.5%
Nickel	1.79	1.86	3.33	62.0 "
Cobalt	2.12	1.90	4.04	67.5 "
Silicon	4.18	0.09	0.377	37.7 "
Mercury	1.73	2.87	4.96	78.5 "
Copper	0.62	4.26	2.63	74.1 "
Gold	0.37	7.62	2.82	85.1 "
Silver	0.18	20.5	3.64	95.0 "
Sodium	0.005	5.22	2.61	99.7 "

The refractive index n has the same significance as for transparent substances; that is, the velocity of light in the metal is given by dividing the velocity in air, viz., 3×10^{10} centimeter-seconds, or 186,000 miles a second, by this index. This means that the actual velocity of propagation of light in metals varies from about 7×10^5 centimeter-seconds, or 45,000 miles a second in the case of silicon, to no less than 1.66×10^{11} centimeter-seconds, or over 1,000,000 miles a second for silver, while in the case of sodium it reaches the enormous value of 37,000,000 miles a second. It may be pointed out in passing that while there is apparently no escape from the conclusion that such metals actually do transmit light with velocities greater than that in a vacuum, the fact is in apparent disagreement with the new theory of relativity as applied to physical phenomena, for this postulates that the maximum possible speed is that of light in free space.

The meaning of the absorption index x is best explained with the aid of the coefficient nx, which has the following significance: When the light has penetrated to a depth of a wave length (measured in air—in this case 0.00059 millimeter) in the metal its intensity is reduced by the factor

$$e^{-4\pi nx}$$

For a greater depth this exponent is to be multiplied by the number of wave lengths in the distance. Taking an average of about 3 for the extinction coefficient nx for the common metals, this means that only 0.0016 of the light penetrates to a depth of even 0.0001 millimeter, while the decimal, which represents the fraction of light passing through a sheet a millimeter in thickness is 1 with 65,000 ciphers before it. Stated in other terms, the absorption of a sheet of metal for yellow light is nearly the same as that of a layer of pure water one billion (10^9) times as thick.

The reflecting powers listed are computed by the simple theoretical formula

$$R = \frac{n^2\ (1+x^2) + 1 - 2n}{n^2\ (1+x^2) + 1 + 2n}$$

from the constants n and x and are found to agree very well with the reflecting powers found by experiment.

As might be expected all these "constants" change with the wave length of light, but in a direction contrary to that shown by transparent substances. For while in the latter case the refractive indexes show in general a decrease with increasing wave length as seen in Fig. 3, most of the metals exhibit anomalous dispersion, that is the index increases with the longer wave lengths. The writer has studied with specially designed spectrobolometric apparatus (Fig. 7) the optical constants of a number of metals as far into the infra red spectrum as wave length 2.5μ, or three times the longest visible wave length, and finds that this tendency of the refractive index to rise still continues even at this point.

In the very short wave lengths of the ultra-violet on the other hand the dispersion as measured by Minor is far from regular, but varies up and down. The index of refraction for silver at wave length 0.316μ goes up to 1.13 and this brings the reflecting power down to 4.2 per cent.

In general the reflecting power of metals is larger in the infra-red, silver and copper reaching values of over 95 per cent at 2μ. An exception to this rule is silicon, whose reflecting power shows a gradual decrease with the longer wave lengths, and in accounting for this we are led to the general explanation of all the anomalies we find in the optical properties of metals. In the modern view of electrical phenomena based on the electron theory it is supposed that metals contain large numbers of electrons which are free to move, and that electrical conduction is brought about by the flow of these negative charges of electricity. Now in addition to the free electrons, metals probably also contain others which are not free to move, the "bound" or "dielectric" electrons, so called because they are supposed to be similar to those in a dielectric; that is, they may be displaced from their mean positions by an electric force, but cannot move through the substance and cause conduction. In proportion then as a metal contains more of these non-conducting electrons will its optical properties resemble more closely those of a dielectric. In the case of silicon this proportion is doubtless relatively high, for its conductivity is low as is its reflecting power also, and moreover this decreases with the longer wave lengths just as in the case of dielectric substances. This mixture of conducting and non-conducting electrons will probably account, then, for most of the peculiarities we find in metal optics, especially for the shorter wave lengths; for when the period of vibration is longer these bound electrons are of less influence—probably their own natural periods are too short—and the reflecting power, as we have already noted, can be computed from the resistance of the metal.

Magneto-optic Effects.—In addition to the properties already noted, the magnetic metals, iron, nickel and cobalt show certain other optical effects on polarized light. Thus, if a thin film of one of these metals is

placed perpendicular to a strong magnetic field, a transmitted beam of plane polarized light will suffer a rotation of its plane of polarization. This is similar to the Faraday rotation exhibited by all transparent substances in a magnetic field, but vastly greater, for it amounts to nearly 200,000 degrees per centimeter thickness for iron, and about the same for cobalt and nickel. There is also a similar rotation, the Kerr effect, which takes place when plane polarized light is reflected from a polished surface of these metals in a magnetic field. We cannot here go into the theory of these rotations save to remark that they are related to the other optical properties of these metals. It may be noted in passing that this rotatory power serves as the best and indeed the only way of measuring the thickness of extremely thin films of the magnetic metals. The writer has measured angles of rotation, given by such films, of hardly more than two thousandths of a degree in magnitude. Assuming that rotation is proportional to thickness for even such thin layers of metal, this would mean a thickness of not more than 10^{6} centimeters, which is about the accepted value of the diameter of a molecule; or in other words, this film, which would, of course, be on glass, would average no more than a single molecule in thickness.

Radiation from Metals.—From our knowledge of the reflecting and absorbing qualities of metals we are able to anticipate something of their radiating properties when hot. Thus experiment agrees with theory in showing that such emission is partially polarized in character when coming from a polished bright surface such as that of platinum.

Because of their high reflecting power and consequent small emissivity, the radiation from metals is much weaker for a given temperature than from a "black body" or perfect radiator. Estimates of temperature based on such radiation will then be too low, and to overcome this difficulty Mendenhall has recently developed a method for measuring such temperatures whereby the metal is bent into the form of a sharp V and the light from the interior studied. This gives nearly the same radiation as a black body at the same temperature and hence affords an accurate determination of the temperature with the aid of an optical pyrometer (Fig. 6). By this means the average burning temperature of the carbon lamp has been determined as 1,950 deg. Cent., of the tantalum as 2,020 deg. Cent., and the tungsten as 2,290 deg. Cent.

Metallic Reflection in Other Substances.—A number of other substances show some of the characteristic optical effects of the metals, particularly in the matter of high reflecting power. Such are the aniline dyes which owe their peculiar colors to an extraordinary absorption for certain colors of the spectrum and consequent high reflecting power for these same colors. This corresponds somewhat to the case of gold, which has a higher absorption for the red end of the spectrum, and consequently shows yellow by reflected light and green by transmitted light. Michelson has recently shown that the colors of a number of birds and insects are "metallic" in nature. But perhaps the most interesting case of metallic reflection in non-metallic substances is that which occurs for certain long wave lengths in the infra-red spectrum for a number of common transparent substances such as quartz and rock salt. Thus, the latter substance shows a very great absorption for radiation of wave length 0.065 millimeter, which absorption is doubtless due to electrons, or more likely ions, whose free period of vibration corresponds to this wave length and which consequently absorb the energy readily. The immediate consequence is a high reflecting power—almost as great as that of metals—for this same wave length, and if radiation containing all wave lengths, such as is given by an ordinary lamp, be reflected a number of times from rock salt surfaces it will be found to consist almost entirely of this one particular wave length of maximum absorption and reflecting power. This "reststrahlen" method of Rubens and Nichols has been widely used to secure nearly monochromatic radiation of such long wave lengths.

Sintering and Briquetting Flue-Dust*

Utilizing a Waste Product of Blast Furnace Operation

By Felix A. Vogel

With the increasing use of fine ores, larger furnaces and high-pressure blast, the production of flue dust is constantly increasing, and amounts annually in the United States to 3,000,000 to 3,500,000 tons, an exceedingly high tonnage, of which a large part has been discarded as valueless. It is generally a fine material containing considerable coke and iron ore, with a small admixture of lime and silica, according to the burden. The iron ore is partly reduced, which shows that it originates largely from the reducing zone of the blast furnace. In the United States the dust usually contains 20 per cent of coke and more than 40 per cent of iron. Estimating coke to be worth $3.25 per ton, and iron ore 7 cents per unit, a ton of flue dust, unless made available, presents a loss to the furnaceman of $3.50 per ton. This accounts for the first efforts to recharge the flue dust into the furnace, either by moistening it down with an excess of water, or mixing it with clay to form balls or pulp, or treating it with lime water.

CONDITIONS IN UTILIZING FLUE DUST.

To recover, in the blast furnace, all the values represented by the material contained in the flue dust, the following conditions should be complied with:

1. The dust should be agglomerated into lumps about the size of furnace coke, so that it will help to carry the burden and facilitate the flow of the gases.
2. The agglomerated material should be strong enough to carry the burden without disintegrating; it should be heavy so as to decrease its volume, and it should be sufficiently porous to permit the furnace gases to penetrate fully. Under no condition should the surface be glazed.
3. It should contain all of the valuable constituents of the dust, such as coke, iron ore, lime, etc.
4. It must stand handling, without undue breakage, and should not produce more than 5 per cent of dust.
5. It must stand the weather.
6. It must not disintegrate in the blast furnace before being greatly or totally reduced.
7. It must submit to easy reduction without requiring additional fuel.
8. It must not contain substances detrimental to blast furnace operations.
9. Its cost of production must be low.

Blast furnace operations, by the use of such agglomerated material, will result in: (1) Regular steady operation; (2) increased burden, increase in the yield of metal produced; (3) decrease in the consumption of coke; (4) decrease in the production of flue dust; (5) decrease in the cost of producing pig iron.

DIFFERENT AGGLOMERATING AND SINTERING PROCESSES.

A number of processes have been evolved in the United States, generally known as agglomerating and sintering processes. They are based on fritting together the particles of ore by heat, the binding action being due to the formation of silicates, mostly of iron, when the coke has not been eliminated mechanically from the dust, it is burned out, leaving ashes in the agglomerated material, which increases the formation of silicates of iron or glazing material. In some cases, however, more fuel is added to the flue dust, which, naturally, further increases this drawback.

The nodulizing process, the oldest process in the United States, has been in successful operation for a number of years. Flue dust is treated either directly, or it is previously submitted to a magnetic separation to eliminate coke and lime. This is done to facilitate the subsequent nodulizing operation, which is carried on in a slowly rotating cement kiln from 80 to 120 feet long. From 200 to 300 pounds of finely powdered coal is used per ton of finished material, the coal being blown into the kiln. Gas and oil have also been tried with more or less success. The heat produced is considerable and difficult to control, the semi-soft material formed consisting of iron ore particles and slag, which, by the revolving action of the kiln, is balled together in nodules of various sizes—from a pinhead to that of a cannon ball—which are usually quite dense, often fused and glazed. They contain 60 to 67 per cent of iron, which makes them quite attractive from the furnaceman's point of view. To make nodules an elaborate and expensive plant is required, the operation of which is more or less difficult and costly.

The Huntington-Heberlein pot process, which has been used with good results in the roasting of pyrites cinders, has been recommended for the fritting of flue dust. The resulting fritted material is of more or less cellular structure. However, this would not be an advantage, as the surface would be largely glazed, rendering it impermeable to gases, and would have to be removed in the blast furnace at about smelting heat. The process is rather simple, requiring stationary iron pots fitted with a perforated false bottom through which the air is blown into the charge. The equipment is cheap; the operation, however, is not continuous, which makes it expensive.

ONE OF THE BRIQUETTING PROCESSES.

The Groendal briquetting process has also been applied to flue dust. The flue dust, either moistened, or after the elimination of the coke and stone, is pressed into bricks which are subsequently fritted in high temperature. To facilitate the operation it was found necessary to eliminate the coke; this, however, increases the cost. The separated coke is of little value, since it contains many impurities. The presses used are ordinary brick presses. The bricks are carefully placed in layers on cars, which are run into long kilns about 170 feet long, where they are submitted to high temperature, gas being used for the purpose. The platforms of the cars are built of fire brick and form the bottom of the kiln. The highest temperature, about 1,300 or 1,400 deg. Cent., is reached in the center of the kiln, the highly burned briquettes working gradually toward the cooler end of the kiln, where they are finally unloaded. The operation lasts about 7 hours. The resultant briquettes contain from 65 to 70 per cent of iron with practically no impurities, these having been eliminated. No ashes are left by the fuel. The briquettes are only slightly fritted, are very porous and friable, and make a high-class material for use in an open-hearth furnace. The manufacturing of Groendal briquettes is expensive, necessitating a costly plant, which has limited output. A ton of flue dust will produce about two-thirds of its weight in briquettes.

Quite recently the Dwight-Lloyd process has been applied to the sintering of flue dust. The material is submitted to internal combustion in layers of from 5 to 7 inches thick. It is fed on an endless conveyor formed of iron pallets or grates. After the fuel therein contained has been ignited by means of a gasoline torch, or some other device, the air is drawn through by suction. The operation lasts about 20 minutes. A good deal has been published of late in regard to this process. It is claimed that the required plant is not costly, while the operating expenses are considerably lower than in the previous process. The resultant sintered material is not homogeneous, and, while a large portion of it is of cellular structure, it is glazed on the surface, which makes it quite difficult to reduce in the blast furnace.

The Greenawalt process uses much the same apparatus as the Heberlein pot, but air is drawn through the charge as in the Dwight-Lloyd process. The sintered material from these various processes is expensive on account of the loss of carbon and, with the exception of the Groendal briquette, is not of good physical structure and is usually glazed on the surface.

BRIQUETTING OF FLUE DUST.

The briquetting of flue dust has been more attractive abroad than has the sintering. The lime process mixes the flue dust with from 5 to 10 per cent of hydrate of lime and, after briquetting, the material is exposed to the air for a certain length of time, so that a carbonate is formed which is the binding medium. This binder will eventually act as flux and replace a certain amount of stone. The process, while having decided advantages, is quite cumbersome and costly, as the briquettes must dry from two to four weeks under cover.

The pioneer process uses sulphide pitch (obtained from the sulphide pulp mills) as a binder. It is an organic substance, rich in carbon and hydrocarbons, which will burn readily and thus increase the calorific value of the material. The flue dust is pressed into briquettes with 4 to 8 per cent of sulphide pitch; they give fair results, but the process is expensive.

The Romay process does not use a binder. The flue dust is submitted to a very high pressure in a specially constructed type of hydraulic press; the resulting briquettes can be handled immediately and have proved very satisfactory. The process requires an expensive plant, however, increasing the operating cost.

The Schumacher process does not use what may properly be called a binder, but is based on the latent cementing actions existing in fresh flue dust and made active by the presence of a small amount of a suitable substance. Thus 0.25 per cent of magnesium chloride mixed with fresh flue dust and 6 to 10 per cent of water, pressed into briquettes, will create a strong reaction, noticeable by the considerable heat developed; the briquettes will be perfectly set and hard within a few hours. The process is very simple, an ordinary pug mill being used in which to mix the material, which is then pressed into briquettes in a toggle press and loaded on cars and allowed to set.

*Paper presented at a recent meeting of the American Institute of Mining Engineers and published in *The Iron Age*.

The Design of Model Propellers and Elastic Motors

By E. A. Vessey

THE science of model flying-machine making has been so fully dealt with through the medium both of separate articles in the various flight journals, and even of small textbooks devoted to the subject, that it seems strange that the really useful information to be obtained concerning the correct design of propellers and motors to suit certain fixed requirements should be so meager. The only article attempting to touch this branch of the subject which has come to the notice of the present writer is one by Mr. W. Langdon Davies on "Twisted Elastic Motors," in which he describes in detail some very clever tests for ascertaining the maximum torque at a two-inch radius to be obtained from elastic skeins of various proportions; and the maximum safe working twist. From these results he deduces the total work of the skein. The information contained in the article referred to is most useful; especially that relative to the safe amount of twist for the elastic, but unfortunately from the model maker's point of view it is no easy matter to apply such information to the actual design of a motor to generate the amount of power required in any given instance to accomplish flight. In scientific model making, as in the manufacture of the full-size flying machine, the question of propulsion resolves itself into the consideration of the thrust required and the velocity of flight, that is, it is first essentially a question of "power" pure and simple. For the sake of clearness it may be well to point out that the term "power" is used in its full mechanical significance as meaning the "rate of doing work," work being defined as "the product of the force exerted and the space passed over." Thus a model flying at 20 feet per second with a mean thrust of ½ ounce will require to be provided with power represented by:

$$\tfrac{1}{2} \times 20 = 10 \text{ foot-ounces per second.}$$

If the model covers 300 feet before the elastic is unwound, the mean work done is represented by:

$$\tfrac{1}{2} \times 300 = 150 \text{ foot-ounces.}$$

Now, Mr. Langdon Davies' figures give us only the work done; but say nothing as to the rate of doing it.

From these remarks it is not to be thought that the present writer wishes to detract in any way from the credit due to the earlier investigator. Such is not by any means the intention, the present article being framed rather as supplementary to Mr. Langdon Davies'; the threads being taken up where he left them.

There is reason to believe that the correct proportioning of the propellers and elastic motors of models to secure maximum efficiency is very little understood, the reason being probably that everything is sacrificed to secure long flights.

The experiments about to be described were commenced many months since, with a view to finding, and if possible putting into some simple form, the laws governing the twisting and untwisting of elastic skeins. It was realized from the first that, in order to secure useful results, the skein and propeller must be tested together, the power exerted by the skein being measured in terms of the thrust and absolute velocity produced when using propellers of varying proportions. The apparatus used will be described later. In comparing various skeins it was necessary, in order to insure that the results should be obtained under similar conditions, that the same relative twist should be given to each skein. It was found that the thrust given by a propeller driven by a twisted elastic skein did not, as might at first be expected, decrease gradually from a maximum to a minimum as the skein unwound, but fell in certain well-defined steps, remaining for relatively long periods at thrust values which were practically constant throughout such periods. The "jump" in thrust was found to take place approximately when the skein was changing from one series of knots to another, the thrust being maintained practically constant throughout the unwinding of any one series of knots. This phenomenon accounts for the fact that a model in flight appears to lose its propulsive power suddenly, and not gradually, as would at first seem rational. Since the thrust and velocity of the propeller are both functions of the speed of rotation, it follows that the flight velocity also decreases in steps, the "jump" in each factor taking place simultaneously with the change from one series of knots in the skein to another. This being so, it was thought best for the purpose of the test to make the standard wind for all skeins not a certain number of turns, but a certain number of completed knots.

When a skein of elastic is wound up, it twists first into a straight stranded rope, which gradually gets tighter and tighter up to a certain point, when a further twisting causes a rapid decrease in the tension with the formation of a knot in the skein. Immediately after the formation of the first knot the tension in the skein again increases, and goes on increasing until the skein is uniformly knotted throughout its length, when a second rapid drop in tension occurs and continues until the first of a second series of knots is formed. With the elastic dry or chalk-lubricated the second series of knots is incomplete when the fracture of the skein takes place; but if lubricated with one of the special preparations provided by various makers, the complete series may be attained, and even part of a third series, though it is difficult to say exactly, by observation, when the second series ends and the third begins. It seemed fairly obvious that the standard wind must be chosen so as to comprise a complete series of knots; and since a great many experimenters prefer to use the elastic untreated, the complete first series of knots was decided upon, and various propellers were tested on standard skeins of varying numbers of strands, the skein always being wound to a complete series of first knots irrespective of the number of strands. In this way a good mean value for the thrust and absolute velocity was considered to be obtained, since obviously, whatever results are obtained by winding the skein, either lubricated or not, to its utmost safe capacity, the thrust and velocity for soaring must still be produced when the first series of knots is in operation, if any length of flight is desired. This method of design would appear to provide an ample surplus of power, when the skein is fully wound, to give the necessary acceleration after launching and to secure reasonably high flights.

The elastic used in most of the tests was of the ordinary 1/16-inch square variety, this being chosen because it can be obtained in very long continuous lengths, also the section is practically uniform both throughout any one sample and in various samples. The strip elastic, as usually sold, is in comparatively short lengths, the width often varies along any one length, and it is difficult to find a series of lengths sufficient to form a skein which are of precisely the same widths. For ordinary practical purposes this does not form a serious objection, but is prohibitive when making any form of test.

The propellers were all cut from solid pine and polished. The working face of the blades was flat and of uniform pitch throughout its length. All propellers were made to standard proportions, which were arrived at after careful trial on actual flying models. The rule finally adopted as giving the best all-round results was:

If w = the width of the block from which the propeller is to be cut,

x = the thickness of ditto,

d = the diameter over propeller tips,

θ = the pitch angle at the blade tips,

Then:

$$w = 0.157d$$

$$\text{and } t = 0.157d \tan \theta$$

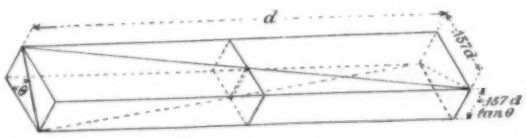


Fig. 1. The Propellor Marked Out for Cutting.

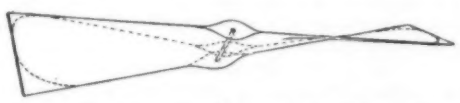


Fig. 2. The Propellor Finished, All But the Tips.

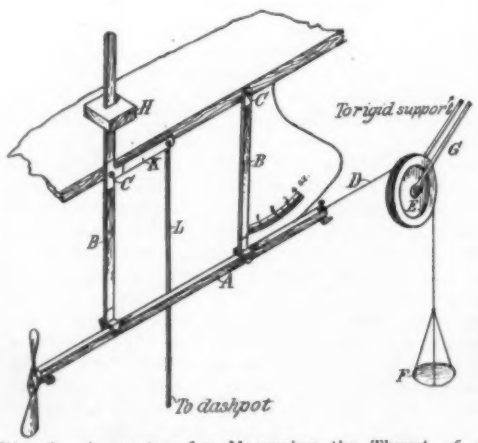


Fig. 3. Apparatus for Measuring the Thrust of a Propeller.

The profile of the blade was of modified Chauvière form with the leading edge curved and the trailing edge straight, the blade back being rounded. The length through the boss of the finished propeller measured along the axis of rotation was made $= \frac{l}{2}$, and the whole of the reduction was made from the trailing side.

Fig. 1 shows the propeller block marked out for cutting, with the chief dimensions shown.

Fig. 2 shows the same block finished with the exception of shaping up to the standard profile, which is marked in dotted lines on the working face.

It will thus be seen that all propellers made to this rule have the same projected area, if the diameter is the same; the projected area varying directly as the square of the diameter on propellers of different diameters.

Such propellers were found to give excellent results under all conditions of practice, and could be run at very high speeds without any falling off in the proportional thrust.

The first test made was to find the law which governs the number of revolutions required to form a complete series of first knots in skeins of various lengths and numbers of strands. It was found that, within very narrow limits, the number of revolutions required is given by the expression:

$$W = 28.4 \frac{l}{N^{0.63}} \quad \ldots \ldots \quad (1)$$

Where W = the required number of revolutions.

l = the length of the skein in inches.

N = the number of strands of 1/16-inch square cord.

Table I gives the results for skeins 12 inches long, corresponding values for similar skeins of greater or less length being obtained by multiplying by the length in feet.

TABLE I.

Number of strands.	1	2	4	6	8	10	12	14	16	18	20
First knot revolutions	341	220	142	110	92	80	71	64	59	55	51

Since the formula gives the number of turns to complete the first series of knots, and since it would appear logical to assume that fracture always occurs at proportional winds, the same formula should give the number of turns in the safe wind, if the constant be altered. It has been thought interesting to compare the results obtained by the equation thus adapted and those already published by Mr. Langdon Davies. The comparison is therefore shown in Table II below.

TABLE II.

No. of strands.....	2	4	6	8	12	16
Langdon Davies...	402	283	234	203	156	143
$W = \frac{K}{N^{.63}}$	485	313	242	202	157	131

The figures given by Mr. Langdon Davies are for skeins 12 inches long and are 75 per cent of the number of turns required to break the skein. In applying the formula it has therefore been necessary to fix a fresh value for factor K. For this purpose the wind for the 8-strand skein has been taken as the mean value and K has been worked out for this. It will be seen that the results agree fairly well. The discrepancy is probably explained by an error in the assumption that fracture occurs in varying skeins at proportional winds, thick skeins apparently bearing more winding than thin ones. Small variations in the initial tension of the skein, and lubrication of the elastic, were found to have no appreciable effect on the above results.

The number of revolutions given by the above formula were adopted as the standard for each skein and used throughout the following tests.

A series of propellers was then made, varying in diameter from 5½ inches up to 12 inches and in pitch angle at the tips from about 19 degrees up to 35 degrees. The apparatus shown in Fig. 3 was then devised for measuring the thrust. The bar A, which carries the propeller bearing and anchor hook for the elastic, was made long enough to accommodate a 30-inch skein (this skein when relaxed was 24 inches long) and was supported so as to permit of its swinging by parallel links B B from the rigid supports C C. Behind one of the links was fixed a scale reading in ounces and fractions the direct thrust on A. The scale was carefully calibrated by hanging known weights in a scale pan suspended from the string D, which passed over

pulley E and was attached to the end of the bar A. The pulley E was made of large diameter and small bore to reduce friction effects to a minimum and was swung from a support F (not shown) by a long link G. By this means the pull of the string was maintained parallel to the bar A throughout a considerable swing and could be finally corrected by raising or lowering F. The aftermost link B was continued above its support and shaped to accommodate a sliding weight H, which could be fixed in any position. This was used to compensate for varying weights of elastic and propellers when changing skeins, etc. The same link was provided with a lever K, more or less at right angles to it and projecting forward, from the outer end of which a long rod led downward to a dash-pot with a moderate damping effect to make the readings as dead beat as possible. To avoid all possible chances of error, the calibration of the scale was repeated on every change of skein and propeller, after the compensating weight H had been set. The scale pan and string were left attached throughout the tests. The propeller spindles were mounted each on a plain bearing, and the thrust was taken on two small flat washers. The duration of the run with each combination of skein and propeller was taken with a stop-watch. The skeins, as has already been said, were invariably wound to a complete set of first knots, and readings were taken for time and thrust for each combination. Three sets of readings were taken in each instance, and the mean values adopted as correct. Readings were also taken with various lengths of skein, from which it was found that, whatever the length of skein, the thrust and velocity remained constant, but the time varied in direct proportion to the length of skein. It was also noted that, whether the skein was run dry or lubricated, the values of the variables remained unaltered; but there is reason to believe that, after the lubricant has thoroughly permeated the rubber, it has the effect of considerably diminishing all the values. Various sections of rubber were tried, and it was found that, so long as the total area of section remained constant, the results were practically unaffected.

The effect of reducing the area of the blade within small limits was tried, and it was found that, provided the diameter of the propeller was kept constant, no appreciable effect resulted. A considerable reduction in the width of blade at the root was also made without appreciable effect.

In this way a number of sets of comparative data were found. The results were then plotted and the mean curve drawn. An equation was then found for each set of variables which satisfied the conditions within the limits of practice. The resulting equations were as follows:

$$T = \frac{19N}{29 - N} \cdot d^{-\frac{1}{2}} (\tan \theta)^{-1} \quad . \quad . \quad . \quad . \quad (2)$$

$$V = 3910 \frac{N^{.37}}{d^{0.5}(40 - N)} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$t = \frac{40 - N}{770N} \cdot d^{2.5} \cdot l \cdot \tan \theta \quad . \quad . \quad . \quad . \quad (4)$$

Where T = maximum thrust in ounces.
N = number of strands of 1/16-inch square elastic cord.
d = diameter over tips of blades in inches.
θ = pitch angle at tips of blade in degrees.
V = mean absolute velocity of propeller in miles per hour.
t = duration of run in seconds.
l = length of stretched skein in inches.

Equations (2) and (4) were derived direct from the test data.

Equation (3) was found from the consideration that:

$$V = k.p. \text{ (r.p.m.)}$$

$$\text{The mean r.p.m.} = \frac{k_1 W}{t}$$

and $p = \pi d \cdot \tan \theta$

$$\text{whence } V = k \pi d \tan \theta \frac{k_1 W}{t} = K \frac{d W \tan \theta}{t} \quad . \quad . \quad . \quad (5)$$

Where K, k, and k_1 are constants and W = the number of turns given in winding up the skein. All other symbols remain as before. By substituting in equation (5) the expressions for W and t given by equations (1) and (4) the final equation in V is arrived at, viz., equation (2) above. It will thus be seen that V, which is called the "mean absolute velocity" of the propeller, is the velocity of translation it would attain if it were working in a solid medium, after the manner of a bolt in a nut. This must be reduced to flight conditions by correcting for slip, but since the slip will, in all probability, vary for each propeller, it has been thought best to present the velocity in this form. Probable values of slip are given later.

Having established the formulæ and checked the same carefully against all the test data, a propeller and motor were designed strictly in accordance with the dimensions given by the equations under the conditions chosen. The propeller was made, and tested, and found correct.

In view of possible criticism by mathematicians, the writer would like to state at once that he makes no claim whatever to absolutely mathematical accuracy in the formulæ given above. As has been said, these are admittedly empirical and have been evolved by a practical man for the use of practical men. The formulæ are sufficiently true within the limits shown in the chart, which is illustrated and described later, but for conditions outside these limits their accuracy cannot be vouched for though there appears no reason to doubt its continuance. The limits have been chosen to cover as far as possible all ordinary practice, and it is thought that occasion will seldom, if ever, occur when it is found necessary to use values for any of the factors in excess of those given.

After long trial the writer has become convinced that the correct design of the propeller and motor for a model is quite as important as it is for a full-sized machine; at any rate, if any comparison is to be drawn between the performance of the model and its full-sized reproduction. It was because of the entire absence of any published information on the subject that this investigation was entered upon.

It must be admitted that the formulæ finally arrived at are somewhat alarming in aspect, owing to the fractional indices. Although the solutions involve the use of comparatively simple mathematics, still there is no doubt the manipulation may be beyond the capacity of many enthusiastic investigators, especially the more juvenile. It has therefore been thought advisable to present the result arrived at in the form of a simple chart, by the use of which all mathematical work is eliminated and the solution becomes merely a process of correctly following certain curves.

The chart is given in Fig. 4, and will be seen to consist of two separate sections. The upper section is plotted with a vertical scale of propeller diameters ranging from 3 inches to 18 inches, and a horizontal scale of mean absolute velocity from 1 to 40 miles per hour. The diagonal lines, sloping upward toward the right hand, show the relation between the thrust and diameter for varying tip angles, and those sloping upward toward the left the relation between the velocity and diameter for varying numbers of strands in the skein. In the lower section the vertical scale gives the number of strands in the skein, and the horizontal scale the maximum thrust in ounces. The curves show the relation between these two factors. To explain the use of the chart, we will suppose that we have a propeller of 8-inch diameter with a tip angle of 26.5 degrees, and we require to know what thrust and velocity it will give us on a 10-strand skein. (N. B.—The skein length makes no difference.) In passing, it must be pointed out that there are of course an infinite number of diagonals; a few only being drawn for the sake of clearness. The others may be imagined as lying parallel to those drawn, and their position may be located by proportionate division. Thus, the diagonal for a tip angle of 26.5 degrees will be approximately at a quarter of the distance between diagonals marked 26 degrees and 28 degrees, and measured from the former. Having found 8 inches, the given diameter, on the left-hand vertical scale of the upper section, we trace horizontally until we meet the imaginary diagonal for 26.5 degrees tip-angle, and then vertically downward to the dividing line between the two sections. The course is shown in dotted lines. If our skein had consisted of one strand, we should have traced on vertically downward, emerging at 0.09 ounce thrust, but the skein has 10 strands, and correction is made for this condition by tracing diagonally parallel to the nearest curve until we meet the horizontal marked 10 on the left-hand scale of the lower section, and then vertically downward, emerging at a thrust of 1.26 ounces, which is the required thrust. To find the velocity, we go back to the upper section, and find that the horizontal dotted trace from 8-inch diameter has already intersected a diagonal sloping upward to the left and marked 10 strands. From this intersection we trace vertically upward, emerging at 13.3 miles per hour, which is the velocity required. This is called the mean absolute velocity, but the actual mean flight velocity may be taken safely as about 10 miles per hour. It is interesting to note that this identical propeller on test gave 1¼ ounces thrust and 13 miles per hour mean absolute velocity. It is of course hardly necessary to point out that all propellers treated by means of this chart must be of the proportions set out earlier in the article and illustrated in Figs. 1 and 2.

Suppose now we have a model which is capable of sustaining a total weight of 2 ounces, at which weight it glides in still air at an angle of about 1 in 6, traversing a distance of about 35 feet in 2½ seconds. Its natural speed is thus $\frac{35}{2\frac{1}{2}} = 14$ feet per second, or about 9.5 miles per hour. For this machine we wish to design the motor and propeller. The gliding angle being 1 in 6, the actual thrust required for soaring will be 2 6 = 1/3 ounce; and since our chart is based on the elastic being wound only to complete the first full series of knots, we do not require to make much allowance over and above this to cover emergencies (such as acceleration and rising) since this is automatically provided when the skein is fully wound. If the elastic is used dry and wound to the Langdon Davies standard

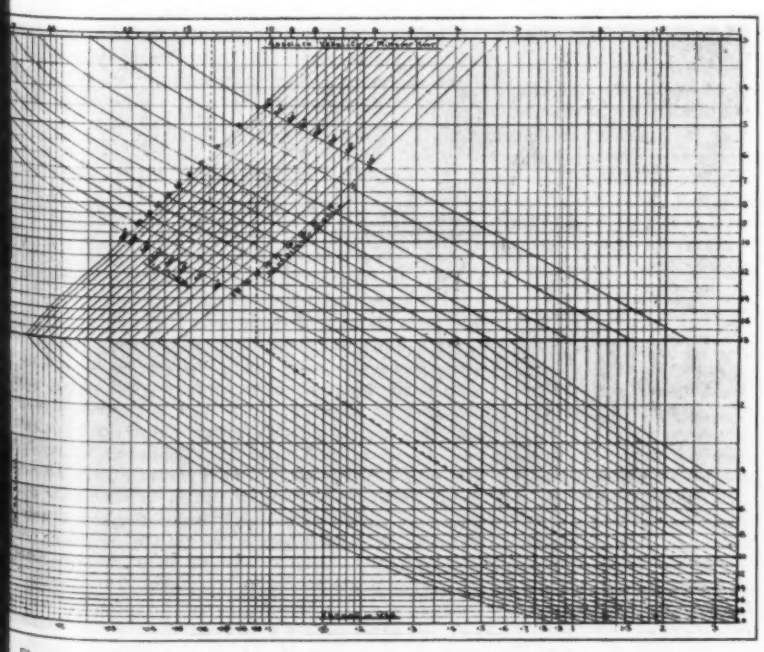


Fig. 4. Chart Showing the Relation Between the Number of Strands in Skein and the Maximum Thrust Obtainable.

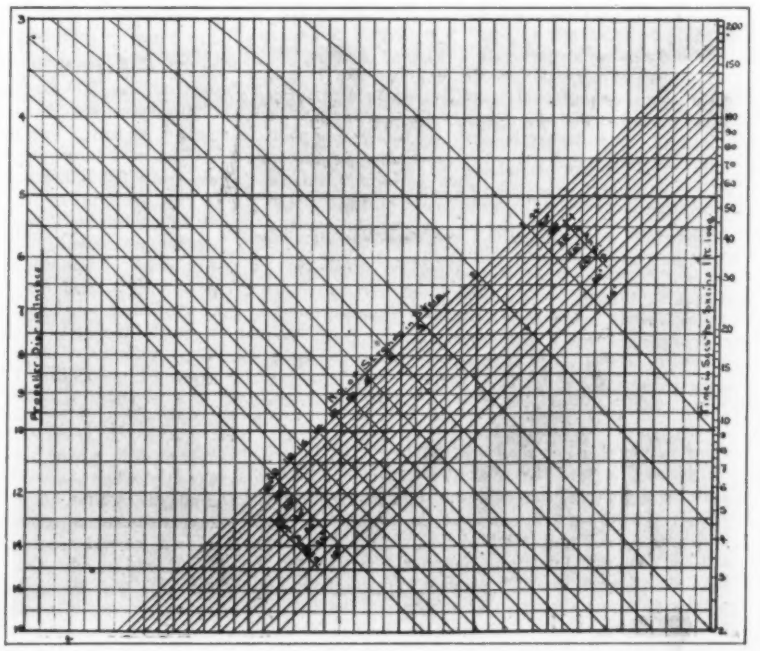


Fig. 5. Chart Showing Time Taken by Various Skeins in Unwinding.

the maximum thrust would be about twice and a quarter that given by the chart, the maximum velocity being about 1.5 times the charted value. In the present case it will be wise to allow for say ½-ounce thrust on the full skein of first knots. Allowing about 30 per cent slip, we find the mean absolute velocity to be about 13.5 miles per hour. With these data let us consult the chart as to all possible combinations which give the desired results. It must be borne in mind that the allowances made above are purely arbitrary, being results of the writer's personal experience. The slip and added thrust may obviously be altered to suit the designer's individual taste and requirements without affecting the argument in any way. Referring to the chart, let us commence with the smallest number of strands, viz., 2. Starting from 13.5 on the velocity scale, we trace vertically downward to intersect with the 2-strand diagonal. This intersection takes place horizontally opposite 4.625 inches on the diameter scale. This, therefore, is the diameter for the propeller to give the desired velocity with two strands of elastic. Let us now see what tip angle we must give it to obtain 0.5-ounce thrust. Starting from 0.5 on the thrust scale on the bottom of the chart, we trace vertically upward to intersect with the horizontal from 2 on the "strands" scale of the lower section, and then from this intersection upward again but parallel to the diagonal curves until we reach the division line of the chart. Thence we trace vertically until we intersect with the horizontal from the diameter already found, viz., 4.625 inches. Through this intersection a tip-angle diagonal will either actually pass or may be imagined. If it is imaginary, its value may be judged by its position relative to the diagonal on either side of it. In the present instance the value is 20½ degrees. Our propeller is therefore 4 5/8-inch diameter with a tip-angle of 20½ degrees. By a similar process of tracing we find propellers for other skeins. Thus:

A 4-strand skein requires a 5 5/8-inch propeller with 32½ degrees tip-angle.

A 6-strand skein requires a 6½-inch propeller with 40 degrees tip-angle.

This is the largest skein we can use, since with 8 strands the tip-angle becomes greater than 45 degrees, which is inadvisable. Let us assume that we can accommodate a skein 30 inches long. In Fig. 5 is given a chart which shows the time taken by various skeins in unwinding, when driving various propellers. The left-hand scale gives the propeller diameter in inches, the right-hand scale the time in seconds during which the completely single-knotted skein 12 inches long unwinds. The diagonals sloping upward to the left refer to the number of strands in the skeins, and those sloping upward to the right, to the tip-angle in degrees. From the chart let us now find out how long each of the above propellers may be expected to run. Taking the smallest diameter first, we find 4.625 on the left-hand scale, and trace horizontally to intersect the skein diagonal marked 2. From this junction we proceed vertically downward to intersect the tip-angle diagonal marked 20½ degrees, and thence horizontally to the right to 5 3/8 seconds on the time scale. This is the time for a single-knotted 12-inch skein of two strands; and since the time varies directly as the skein length, it becomes for a 30-inch skein 5 3/8 × 30/12 = 13½ seconds. If the skein is wound up to the Langdon Davies standard for unlubricated elastic, the duration of the run is increased by 50 per cent over the charted values. Hence the time of unwinding of the 4 5/8-inch × 20½-degree × 2 strand combination is about 20 seconds. Similarly, the times for the 5 5/8-inch × 32½ × 4-strand, and 6½-inch × 40-degree × 6-strand combinations are respectively 27 and 32 seconds.

Now, these are all combinations of equal power, since they give practically the same thrust and velocity in each case. The power-to-weight ratio is given by the expression $\frac{T \times V}{\text{No. of strands}}$ for skeins varying only as regards number of strands, where T = thrust, and V = velocity.

Therefore, comparing the three examples above, we find the power-to-weight ratio to be:

(1) $\frac{T \times V}{2} = \frac{.5 \times 13.5}{2} = \frac{6.75}{2} = 3.375$

(2) $\frac{6.75}{4} = 1.6875$

(3) $\frac{6.75}{6} = 1.125$

Whence combination No. 1 is the best. If, however, we desire to secure the greatest duration of flight, we must use combination No. 3. Similarly, since the velocity is the same throughout the series, No. 3 will give the longest flight, provided skeins of the same length are used in each instance. Suppose, however, for the sake of argument, that it is possible to make the weights of the first two skeins equal to that of the third by lengthening them; thus, making No. 1 skein 90 inches long, and No. 2, 45 inches long. How do the distance and duration values now compare with No. 3 combination? Since the time is directly proportional to the length of skein, the new 2-strand skein will now run 60 seconds, and the new 4-strand skein 40 seconds, which shows that, weight for weight, long skeins of few strands are to be preferred to short skeins of many strands, provided always that propellers to suit are provided. It must not be thought that the writer is hereby recommending a skein of 90 inches long for a 2-ounce model; that of course is impracticable. The point he wishes to make is that on all counts, it appears to be the best practice in design to use the longest skeins possible with the fewest strands and small propellers. With the first assertion all model makers will probably agree, but as regards the other two there will, without doubt, be some difference of opinion. In ordinary full-size theory, everything points to the advisability of using large propellers of high pitch ratio, yet how very seldom are such propellers seen in practice. The question of weight in the prime mover and transmission sets the limit. The only way as yet discovered to keep the motor weight down is to run it at a high speed, which of course necessitates the use of a propeller of relatively small pitch and diameter, with corresponding loss in efficiency. Geared propellers have been used, but the unavoidable complication involved in their employment seriously discounts the gain in efficiency in the propeller itself. It therefore is evident that the point at present to be considered in full-sized design is, by force of circumstances, expediency rather than efficiency, although it is not for a moment suggested that general all-round efficiency should be ignored. As with the full-sized machine, so with the model. To obtain the best results with the materials at hand, we must seek the best combination; and, as has been shown, such does not necessarily include the most efficient propeller. The writer's aim is to secure the best all-round combination, which, in his opinion, is such as will give the longest reasonable flight both as regards distance and duration with the smallest motor, i. e., the least amount of elastic. It is admitted that it is quite possible to select one item in the combination and show that it is not theoretically most efficient, just as has been shown to be the case with the full-sized machine; but the whole of the tests go to prove that the happy medium aimed at has been found.

Now, just a word as to efficiency and "slip." Efficiency is defined as the ratio between the useful horse-power (which is directly proportional to the product of the thrust and the velocity forward of the propeller) and the actual brake horse-power of the motor. Treating the latter first, the power of an elastic motor is a very variable quantity, since it depends, all other things being equal, on the rate at which it unwinds. This may easily be realized when it is remembered that in winding up a certain skein a given number of turns a certain fixed amount of work is done, and this amount of work, with certain very small deductions for friction losses, will be delivered up again by the skein in untwisting. Now "power," as has been already stated, is the "rate of doing work;" hence it is not difficult to see that power depends entirely on the speed of untwisting. In other words, an elastic motor to give maximum power with minimum weight must necessarily unwind at a high speed. This being so, the claim made above, that the best combination comprises a relatively small propeller, is amply borne out, since a precisely similar line has to be followed in full-sized design when dealing with high-speed gasoline motors. Suppose that the work given out by the skein in untwisting is represented by W and the time taken is called t, then neglecting friction losses the brake power may be represented by the expression brake power $= \frac{W}{t}$. For instance, say the skein is capable of performing, say, 500 foot ounces of work. For the sake of argument, say it completely unwinds in 10 seconds, when a certain propeller is used; the power is therefore 500/10 = 50 foot ounces per second. But supposing this propeller is changed for another which only allows of unwinding in say 60 seconds, then the power or rate at which the work is performed is 500/60 = 8.3 foot ounces per second only. We may, therefore, increase the power of our skein by putting faster-running propellers to it, and *vice versa*. So much for the brake power; now as to the useful power. This is proportional to the product $T \times V$ where T = thrust and V = velocity of flight. The efficiency is, therefore, given by the ratio:

$$\frac{\text{Useful power}}{\text{brake power}} \text{ which equals } k \cdot \frac{T V t}{W}$$

Where k is a constant. Since our object is to find the most efficient propeller for a given number of strands of elastic, we may assume W as constant. The expression therefore becomes: Efficiency = $KTVt$ (where K is a constant including W).

Substituting for the various factors from equations (2), (3), and (4) and remembering that in all these equations N is constant, we obtain: Efficiency = $K_1 \frac{l}{\sqrt[3]{d}}$ (where K_1 is a constant including N). Whence we see that tip-angle has no effect on the efficiency, and that a long skein and small propeller are imperative if high efficiency is desired.

In the above investigation one correction is necessary. V in the formulæ should be the actual velocity of flight, and not, as we have made it, by substituting the value given by equation (3), the mean absolute velocity. To make this correction we must make a certain deduction to account for the "slip" of the propeller. Now, the slip is a variable quantity, i. e., it is not only different for different propellers, but varies for any given propeller under differing conditions of load. As far as the writer is aware, no reliable means of calculating this quantity have at present been published, though tests are now being made with propellers under flight conditions with a view to establishing the necessary data. As the matter stands at present, the only course appears to be to assume a safe value, and 30 per cent appears to be a fairly generally accepted all-round figure. Adopting this value, the expression for efficiency becomes

$$\text{Efficiency} = \cdot 7K_1 \frac{l}{\sqrt[3]{d}}$$

It must be pointed out that the inclusion of this slip allowance in no way affects the general argument, since it merely alters the constant K_1.

As has already been said, the above argument proves that the tip angle of the blade has no effect on the efficiency of the propeller when driven by an elastic motor; consequently, in designing we should naturally select a fairly steep angle in order to secure the requisite velocity with a small propeller diameter, and also maximum duration of flight and distance. A pitch ratio of 3:1 will give us a tip-angle of about 43½ degrees, which is in the writer's opinion larger than is advisable. Better results are obtained by using a maximum pitch ratio of 2.5:1, which requires a tip-angle of 38½ degrees, though a larger angle may of course be used in exceptional cases. Having decided on the angle, it is very easy from the chart to settle the diameter and number of strands required to secure the velocity and thrust called for by the model. The propeller must then be made to the proportions set down earlier in the article.

In conclusion, the writer would like to state that the method of design and construction here described is not claimed as being the "one and only" way, but propellers so designed have been found to give excellent results in practice; and further, it is, so far as is personally known, the only detailed method of design yet published and as such, should prove of interest and use both to those uninitiated in the mysteries of the theory of Aeronautics, and it is hoped also to their more technical brethren.

Oil in Boilers

"When the boiler-feed water is taken from the hotwell of a jet condenser," "says *Power*," "as is the practice where fresh water is available for condensing, there is little danger that excessive quantities of oil will be carried to the boiler. There is believed to be little danger to any type of boiler from the oil itself because, being lighter than the water, it will not sink to the bottom and become attached to the plates. The error in this belief lies in the assumption that what is oil when it enters the boiler remains oil. It does not. Subjected to the long continued high temperature of the water, it is slowly distilled, that portion which is volatilized going off with the steam, leaving behind a residue of a higher specific gravity than water, which sinks to the bottom as soon as the circulation stops. When cold it is firm and non-adhesive, but when heated is not unlike asphaltum.

"So long as the circulation continues, this matter will travel along with the current and do no harm. When it settles on the comparatively warm sheet it adheres to it so strongly that it is not dislodged when the circulation starts again. Water is kept from contact with the sheet and a 'bag' is the result.

"When a bag is caused by oil it is liable to be anywhere on the shell below the fire line. It is usually near the bottom, but may be well up on the side and anywhere between the front and rear heads.

"The mass that clings to the sheet is not always entirely oil residual. It is often found agglomerated with the scale-making solids of the water in irregular masses in all parts of the boiler.

"What is really more dangerous, because less liable to be suspected, is the milky-colored emulsion which goes to the boiler after the heavier-bodied oil has been removed by filtration. This does not behave like oil proper in associating with foreign matter nor by gathering in large or small masses, but goes atom by atom to the shell of the boiler, which it covers with a tissue of hard, bright, black varnish, which as effectually prevents the contact of water as the thickest coat of scale. It covers the entire sheet below the fire line before it is noticed and the 'bag' is often as wide as the diameter of the boiler and as long as the sheet. This bagging is gradual, as at the first slight stretch the varnish cracks and the water cools the softened sheet and restores its strength until the coat of varnish again covers it; this is repeated until the sheets are bulged like a barrel."

Soaring Flight

A Study of Bird Flight in Its Relation to the Aeroplane

By R. G. Ecob

IN considering the subject of soaring flight, with a view to its application to aeroplanes, let us start with a rough analogy between the most efficient, up-to-date aeroplanes of to-day, and the soaring birds.

We will assume a composite aeroplane, say of 50 horsepower, weighing with the operator 1,000 pounds, and with a supporting area of 250 square feet. As a matter of fact an aeroplane with these specifications would be overloaded, but we will assume that it is so efficient that it will develop a speed of 60 miles per hour. We will also suppose that 200 turkey buzzards are harnessed together by rigid rods, and are flying as a solid unit. The total weight of such a unit would be 1,000 pounds, the total wing area 1,060 square feet, and the total power 3 horsepower, except when the birds are soaring, when of course it would be zero. It does not take much of a mathematician to see that there is something radically wrong with the modern aeroplane.

Several theories have been advanced to explain the phenomenon of the soaring bird. It is easy to see that the vibrating wing theory is untenable, from a common sense point of view. When a bird is soaring so close to the observer that the quills of each individual feather are distinctly visible, and photographs taken in 1/100 second show no vibration, it is safe to say that there is none. The muscles of a bird's wing are unsuited to such rapid vibration, and are physically incapable of producing it.

Another theory is that the birds take advantage of rising currents of air, or glide from strata to strata of air, extracting their power from their different relative velocities. In observing clouds, smoke from chimneys which has become cold, or haze caused by forest fires, none of the updrafts alluded to are noticeable, and the stratas of differing velocities are very far apart. In crossing any of the rivers about New York on a windy day, the smoke from the river craft will be whipped out in a horizontal line, yet hundreds of sea gulls are soaring continually in the same breeze. It seems strange that the alleged updrafts of air, and air currents of differing velocities never seem to molest the smoke, yet always seem to be convenient for a bird too lazy to flap his wings. The flight of a soaring bird is too positive, and the evident power too great and constantly applied to claim that it is derived from such unreliable sources.

The real solution of the problem lies in the way the bird, by some sort of manipulation utilizes the forces of the wind in conjunction with his inertia and momentum, much in the same way as a sail boat in tacking utilizes the force of the wind in conjunction with the resistance of the water.

A bird by soaring cannot advance into the teeth of a wind indefinitely without coasting down hill. If he wishes to travel against the wind he accomplishes his purpose by circling to gain altitude, and then glides ahead at a remarkably slight gliding angle.

The bird also cannot glide indefinitely with the wind, unless of course it is so strong that he is bodily borne along with it, as a house is carried bodily by a hurricane. As soon as the bird is traveling as fast as the wind, the air is no longer compressed under his wings, and he has to coast down to gain speed, or circle for height.

The crux of the situation seems to lie in the explanation of this circling for height. The dotted line in Fig. 1 is a perspective of a true spiral, the plan of which is a circle, and the rate of ascent uniform. The level of the eye is on the line *X*—*Y*. The dot-and-dash line is the supposed course of a bird. It must be noted at the outset that in watching a bird it would be impossible to plot his exact course on account of the complexity of the figure, and because the amount of rise and fall can not be readily noticed except when the bird is flying on or near the eye level. It can be readily seen by consulting the figure how futile it would be to lay out such a course by merely watching a bird, even supposing that he were flying in true circles, and that the circles were all of the same size.

It is easy to see, however, by watching birds at close range for short periods of time that their real course in executing such a spiral is much the same as the dot-and-dash line in Fig. 1, although probably not so exaggerated.

Fig. 5 represents one cycle of this spiral. When the bird turns to advance with the wind, from *m* to *n*, his course is slightly downward. The stress diagram is represented in Fig. 2. The angle θ is the angle between the chord of the wing *g h* and the horizontal line *A B*, which may or may not be the actual angle of descent. The forces acting on the bird are, the wind pressure and momentum *a–b*, gravity *b–e*, and friction which is not

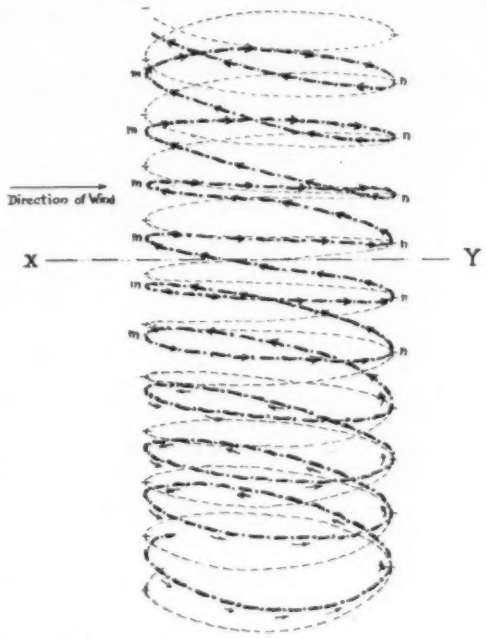


Fig. 1.

True Spiral (Dotted Line) and Course of Soaring Bird (Dot-and-dash).

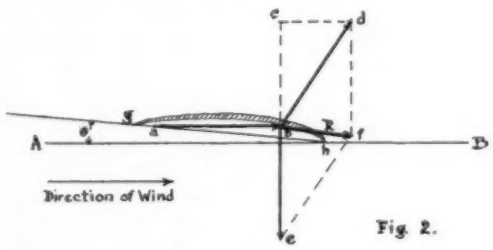


Fig. 2.

Stress Diagram on Turning to Advance With the Wind.

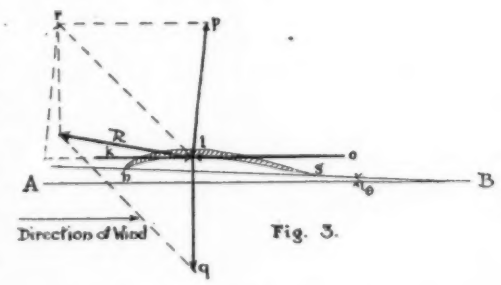


Fig. 3.

Stress Diagram on Turning Into the Wind.

shown. *a–b* acts on the wing to produce uplift *b–c*, and drift *c–d*. The resultant of these forces is *b–f*. These stresses are continually changing as the bird makes his circle. As he progresses from *m* to *n*, Fig. 5, *a–b* decreases as he catches up with the air current, *b–c* remains nearly constant and *b–f* gradually decreases. When he has reached *n*, which we will say is the point where he can no longer find support without gliding down, or using his momentum in gliding across the air stream, he turns into the wind, and changes the angle of incidence of his wings. The forces are then changed as in Fig. 3, which hardly needs explanation as it is the same diagram as would represent an ordinary aeroplane rising under its own power, the force *a–l* being the force of momentum derived from the wind blowing on the under side of the wings while the bird was traveling from *m* to *n*. The amount of the drop from *m* to *n* is made up by the momentum gained by the fall between these two points, and the amount of lift from any point *m*, to the next one higher is gained by the difference in wind pressure, that is, in the half circle from *m* to *n* more wing area is exposed than from *n* to *m*. Here again this theory can not be substantiated by observation, as the eye is not sufficiently accurate to detect the very slight changes in the angle of incidence. In Fig. 2 the angle θ is arbitrarily drawn at 5 degrees, in Fig. 3, 2 degrees. Without the guide lines it would be hard to distinguish them. That these angles do change, not only to accomplish the phenomenon described, but to accommodate the slight gusts and variations of the wind, may be noticed when a bird is stationary relative to the earth, and his wings are seen to turn, and warp.

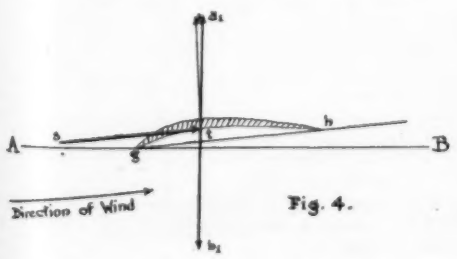


Fig. 4.

Stress Diagram in Upward Wind.

It is doubtful if this feat may be accomplished unless the bird is in a fairly swift current of air, which has an upward trend. In such a case the diagram may be shown as in Fig. 4.

It must be noted that there is considerable variation of speed in making an ascending spiral. Relative to the earth the speed from *z* to *w* is maximum, (Fig. 5). From *w* to *v* it is very slightly reduced, and from *v* to *u* it diminishes until at *u* it is minimum. From *u* to z_1 there is an acceleration. The points *z*, *w*, *v*, *u* and z_1 cannot of course be definitely located; they merely indicate approximately where changes take place. The impulse at *m* is very noticeable when the bird first banks against the wind.

Variations occur in the method of soaring owing to variations in the velocity of the wind. The above illustrations all represent the wind as being moderate, and assume that the sole object of the bird is to gain altitude. Hawks soaring over a valley may combine the circling with a steady trend in any direction. In strong winds the distance *v–u* is very short and steep, while the distance *z–w* is correspondingly long and the slope gradual. In this way the stretch from *v* to *u* is often reversed, that is the friction on the bird is so great that the momentum is not strong enough to overcome it and the bird takes the course *v–*v_1. In this way sea birds are often carried far inland, and while they are able to ride out the gale, they are not able to progress against it either by gliding or by flapping their wings. In very gentle breezes the bird is more likely to help himself along with a few flaps of his wings, than he is in an ordinary wind.

It is doubtful if the above principles can ever produce a commercial soaring glider. It would be almost impossible to start such a glider from even a steep hillside, because the glide from *z* to *w* would bring it back to its starting point, and the machine itself would be much less efficient than a bird.

One may readily imagine a correctly designed aeroplane with a low power engine flying to some height and remaining indefinitely in the air with the motor dead. Such a machine would not necessarily have the appearance of a bird. The automobile engine does not resemble a horse, and even if one were made to work on the principles of a horse's muscular action it would be a complete failure. Why then should an aeroplane resemble a bird?

So in designing a soaring aeroplane it would be necessary to determine with full size models, on specially constructed instruments, the exact direction and strength of the wind forces acting on the planes at various degrees of inclination, the best curve, the best number of planes, their dimensions, and their disposition to the best advantage, the amount of power required, and the amount of weight allowable with a given surface. To this knowledge should be added about 90 per cent of practical experience and common sense.

Motor Car Statistics.—America, cars in use, 500,000; annual production, 130,000. Great Britain, cars in use, 150,000; annual production, 25,000. France, cars in use, 85,000; annual production, 40,000. Germany, cars in use, 50,000; annual production, 15,000. Italy, cars in use, 15,000; annual production, 5,000.—*Allg. Automobil Ztg.*

Fig 5.

Single Turn in Ascending Spiral of Soaring Bird.

Preventing Disease-infection by Oysters.

IT is known that one of the best methods to prevent diseases being caused by oysters is to keep them for some time in pure sea water which is often renewed. To carry this out practically, M. Fabre-Domergue proposes to use basins having a filtered sea-water circulation in them, and thinks that these could very well be used in the oyster industry, doing away with various difficulties. But much care needs to be taken in laying out such basins so as to have healthy oysters without lessening their value at the same time. He shows that in his experiments the oysters left for 8 days in filtered sea-water basins do not lose weight, fleshiness or vital resistance, and the length of time can be doubled without any harm. M. Bodin, in some late experiments, uses artificial sea-water of a composition very nearly that of natural sea-water, but having a slightly less density. The water is taken out of the basin by a pump and goes to a sand filter, then returning to the basin and making a closed cycle. The water is entirely renewed in 1½ hours and this keeps up for 12 hours out of 24. He observed 500 oysters, in lots of 100 specimens, and these were treated for varying lengths of time. After 8 days in artificial sea-water the oysters showed no signs of ill-effects, and the same result was observed after 15 and 30 days in other tests. The weight and the taste of the oyster are little changed after 8 days, but after a longer time it is found that the taste becomes less salty than in ordinary oysters. It thus appears evident that artificial sea-water can be used, and this has the advantage of allowing the basins to be installed at inland points.

Fish Psychology

WHETHER fish are capable of reasoning or of associating ideas in the way which is often so remarkable among the higher animals is still an unsettled point. Some recent experiments made by M. Oxner and presented to the Academie des Sciences throw some light on the matter. Edinger denied that fish can associate ideas, and exact researches on this point have been lacking. It is claimed that a fish has no memory, since after he is hooked for the first time he does not profit by the experience, but can be caught again when the hook is well concealed. The author's work in the laboratory of the Monaco oceanographic establishment led him to another conclusion. First he used a well-concealed hook, and could then catch the same fish day after day; but this proves nothing more than that the hook was so well concealed that the fish was not aware of its presence, and since he thus found no difference between the bait and ordinary food, there was nothing to prevent him from taking the same bait. In another series of trials, Oxner also concealed the hook, but two inches above it he attached a small paper square. In each tank he placed a single specimen of *Coris julis* which had been recently captured in the net. For several days it refused the bait, but on the eighth day the fish bit, and after unhooking it, Oxner replaced it in the water. On the three following days the fish also bit and was caught, but on the twelfth day it no longer touched the bait, and the paper signal began to produce its effect. On the same day the paper was removed, and then the fish took the bait. On replacing the paper, the fish refused to bite for three days, after examining the bait several times. On the next day it approached the paper square, then descended and very carefully nibbled the bait until it had eaten it all off the hook, but without swallowing the latter. This it kept up for a number of days following. M. Oxner repeated the same kind of experiment many times with fish, and the result was the same. He concludes that at first the fish is indifferent and it takes the bait, but the association of ideas of the pain caused by the hook and of the color of the paper commence to act after a few days. The result is that the paper acts as a warning signal in spite of the attraction caused by the food. But by degrees the instinctive action of approaching the food overcomes the prohibitive effect and the fish begins to take the food, but very slowly, and finding that this succeeds, it continues to follow the same action.

A Bread of the Time of the Vikings.—Dr. Schnittger, professor at Stockholm University, has made an interesting find relating to the remote past of his country at Ljunga, in Eastern Gothland, viz., some bread dating from the time of the Vikings. Microscopical examination has shown this bread to be made from pine bark and pea meat, thus proving the fact that peas were grown in Sweden as far back as a thousand years ago. Archaeological excavation has so far brought to light only a few specimens of bread dating from ancient or prehistoric times. The few loaves excavated in Egypt and in Swiss lake dwellings are of the highest archaeological interest. In the northern countries only one or two finds of this kind have so far been made, foremost among which should be mentioned a corn-meal loaf dating from the fourth century A. D., which was discovered by Dr. Schnittger in 1908 in connection with the excavation of Boberg castle.

Science Notes

Among the prizes proposed by the Paris Academy of Sciences for 1913 may be noticed the Pierre Guzman Prize, amounting to 100,000 francs, which is offered for the discovery of a means of communication with a star, other than the planet Mars. The naiveté of the restriction is delightful, says *The Athenaeum*, as if communication with Mars was so easy, that it may be virtually looked upon as an accomplished fact.

Calcium Carbide as a Means of Thawing Out Frozen Pipes.—The fire department of Brunswick, Germany, has recently made exhaustive experiments with calcium carbide to ascertain its practicability for thawing out frozen pipes and water hydrants. The carbide was sprinkled upon the snow or ice covered pipes or hydrants and the gas evolved was lighted. It is stated that one of the hydrants, which was frozen exceptionally hard, and besides covered with a thick coat of ice, was thawed out in fifteen minutes. However, this method is not without danger, and should not be employed by inexperienced persons.—*Carbid und Acetylen.*

Geological Survey Expedition to Siberia.—According to the *Russian Trade Gazette*, the Minister of Trade and Commerce has decided to send out two expeditions to Siberia with the object of carrying out a geological survey and at the same time of reporting on the commercial possibilities of the districts. One expedition is to report on the salt deposits in the district of the Minnusinki Salt Sea, while the other is going to the districts through which the Amur Railway will pass, in order to examine the coal deposits which have recently been discovered between Bureja and Sawitucha railway stations.

International Safety Congress.—An International Safety Congress will be held in Milan, Italy, for five days, beginning May 27th, 1912. This congress, the first of its kind of international scope ever held, will be for the purpose of setting in motion a world-wide movement for the conservation of human life in industry. The American Museum of Safety in New York city is making preparations to the end that the United States shall be well represented. An American national committee has been selected by the American Museum to co-operate with the international body, and to promote the American idea and views at the congress. Dr. W. H. Tolman, director of the American Museum of Safety, and other members of the committee, will attend the congress.—*Elec. World.*

International Conference on Public Baths.—An international conference on people's baths and school baths will be held this year at Scheveningen, The Hague, during the last week of August. Municipal authorities and civic improvement societies in all countries will be invited to send delegates to attend the conference. It is proposed to hold four sessions, embracing the following subjects: (1) Reading of essays, reports and papers regarding people's baths, school baths, factory baths, military baths, and sea baths. (2) Discussions of technical questions relating to the water supply, the planning and general arrangement, the operation and the cost of maintenance. (3) Consideration of the attitude of the state, the province or country, and the municipal community toward this social problem. (4) Relation between public baths and public health. Dr. William P. Gerhard, a consulting and sanitary engineer of Brooklyn, will represent the United States, and will organize here a national committee.—*Eng. Record.*

Artificial Silk from Animal Flesh.—Uses have been found for various parts of dead animals (horses, etc.); the skin is used for the preparation of leather, the fat is used as a lubricant, and the bones are used for the manufacture of glue and certain black dyes. The remaining carcass, however, hitherto had to be thrown away. But it seems that a use has now been found for the flesh also. According to a *Neueste Erfindungen und Erfahrungen*, chemists have succeeded in using it for the manufacture of artificial silk. The mode of preparation is quite simple. The flesh is treated with acids which disintegrate it into its ultimate fibers. The latter are then subjected to a sort of tanning process which imparts to them a silky appearance, at the same time giving it durability. The fibers thus obtained are about 5 centimeters (2 inches) long and look very much like the raw cocoon threads of the wild silkworm (bombyx). These fibers may be vulcanized like genuine silk. If placed in a caoutchouc bath and subjected to a pressure of about four atmospheres for two hours, they can be made perfectly air and water-tight. They therefore appear particularly well adapted for use as insulating material, for pneumatic tires and balloon envelopes, for which genuine silk cannot be used on account of its expensiveness. Attempts to spin these fibers into threads have so far been unsuccessful; however, modern ingenuity will doubtless overcome these difficulties before very long.

Trade Notes and Formulæ.

Brown Paint for Iron.—Forty parts water, 10 parts tannin, 20 parts trichloride of antimony.—*N. Erf. u. Erf.*

Dressing for Driving Belts.—Melt 800 parts of tallow in 4,000 parts of castor oil and to this mass, while warm, add 16 parts gum powder and 80 parts of finely pulverized borax. This dressing can be applied with a soft brush to both sides of the belt. In older belts the dressing can be applied on the flesh side. It is advisable to dress driving belts in use frequently, but they must always first be well washed and cleansed.—*—Ledertechnische Rundschau.*

Rubber Substitute for Stearine Wax.—For the insulation of high tension cables, in connection with other wax or asphalt-like products, a rubber substitute made from stearine wax is used. It is made as follows: To the melted, thin fluid stearine wax about 20 per cent of flowers of sulphur is added and the mass, constantly stirring, is heated to 150 deg. Cent. until the sulphur is melted and the fluid mass begins to boil up. The temperature of 150 deg. Cent. is the limit for wool-wax stearine. In the case of waxes with a higher melting point, the temperature must be raised proportionately, or the heating effected in an autoclave.—*Techn. Rundschau.*

Erasing fluid consists as a rule of two fluids, one of which is javelle water and the other two per cent acetic acid. The ink writing is first painted with the javelle water and then with the acetic acid and then dried off with blotting paper. For an erasing fluid that can be used with all gall nut and log-wood inks, the following recipe is recommended. Oxalic acid, 80 parts; hyposulphite of soda, 20 parts; water, 1,000 parts. A little erasing fluid is placed on the writing or blot to be removed and allowed to work for a few minutes, after which it is dried with clean blotting paper. If necessary, this operation may be repeated two or three times. Any remaining ink or traces of writing can then be removed with the greatest ease. This erasing fluid does not attack the paper. An erasing fluid prepared by Adolf Lenz is a filtered solution of chloride of lime in two parts of water.—*Pharm. Post, Vienna.*

Illuminated Manuscripts.—According to a paper read recently before the Royal Society of Arts, the earliest existing manuscripts, which formed the foundation of European manuscripts, are of Egyptian origin, and some of them are beautifully illuminated, or ornamented with pictures. Until the second century B. C. papyrus remained the chief substance upon which the writings were made, but at that time a scarcity of supply occurred, and Eumenes II., King of Pergamum, introduced vellum, prepared from calfskin as a substitute. Vellum quickly superseded the brittle fiber of the Nile reed and became, as it still remains, the ideal material for writing and illuminating. In 330 A. D. the Emperor Constantine went to Byzantium, and the great early epoch of Christian art began shortly afterward. The Byzantine School was pictorial, the Celtic is ornamental. For about 400 years Irish scribes and illuminators produced magnificent manuscripts, a few of which still exist. English work from the eighth to the twelfth century shows Byzantine and Celtic feeling, combined with other influences. Anglo-Saxon work is noteworthy for the curious outlining and the peculiar attitudes of the figures. In the tenth century there was a great output of beautiful work; in the twelfth century the work generally tended to become smaller and more delicate. In the fourteenth century, the highest point of excellence in English illumination was reached. The fifteenth century saw the decline and practically the end of the art of English illumination, and later work of this kind done there was chiefly the work of foreign artists, mainly Dutch.

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